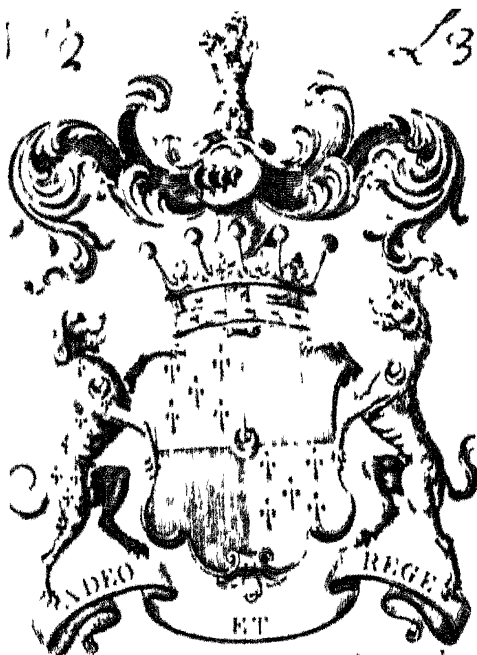


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Philip Earl. Hanhope



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL TRANSACTIONS.

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. XLII. For the Years 1742. and 1743.

L O N D O N :

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IARI

T O

MARTIN FOLKES, Esq;
P R E S I D E N T

Of the ROYAL SOCIETY of LONDON,
For improving Natural Knowledge,
And Member of the ROYAL ACADEMY of SCIENCES
at P A R I S, &c.

S I R,

THOUGH the ROYAL SOCIETY heard with the greatest Concern the Resolution taken by their late worthy PRESIDENT, to decline being any longer chosen into that Office: As they were truly sensible of the many Services he had done them, and still gratefully acknowledge the Continuance of his Favours: Their Loss was immediately made up, and all their Apprehensions removed upon your Acceptance of that Post, in which you have shewn the SOCIETY, that you make their Wellfare and Prosperity the Object of your constant Care and Application. The great Pains you have taken to enlarge their Correspondence both at Home and Abroad, the generous Encouragement you have given to Persons engaged in all the several Branches of useful Knowledge

lege to bring in their Discoveries, the Part you have borne yourself by imparting curious and useful Papers, and the constant Care you are pleased to take of all their Affairs; are the most convincing Proofs of your Zeal and Affection for the ROYAL SOCIETY, and of their Happiness, in having made Choice of a Person in all Respects so qualified to fill their Chair; whereby they have the Satisfaction of seeing themselves flourish more and more, and Arts and Sciences continually encouraged, under the Protection of a Gentleman universally acquainted with all the Branches of Learning, which are the Objects of their Pursuits and Inquiries. Give me Leave, Sir, to make this public Acknowledgement, and at the same time to record my own Gratitude for the many Favours and Instances of Friendship you have conferred upon,

*Dartmouth street,
Westminster,
Nov. 26. 1744.*

*Honoured S I R,
Your most obedient and
Most humble Servant,*

CROMWELL MORTIMER, *M. D.*

*Secretary to the ROYAL SOCIETY, and
Fellow of the ROYAL COLLEGE
of PHYSICIANS.*

*Fig
the first*



113 Blackwell, delin et sculp.

*Fig.
the second*



PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*, 1741-2.

The CONTENTS.

- I. *Extract of a Letter from Mr. Christopher Mason, concerning a Fire-ball seen in the Air, and a great Explosion heard, Dec. 11. 1741. communicated by Nic. Mann, Esq;*
- II. *A Letter from Edward Milward, M.D. to Martin Folkes, Esq; Pr. R. S. concerning an Antidote to the Indian Poison in the West-Indies.*
- III. *A Letter from Edward Nourse, F. R. S. one of the Surgeons to St. Bartholomew's Hospital, to the President and Fellows of the ROYAL SOCIETY, giving an Account of several Stones found in Bags, in the Bladder of one Mr. Gardiner.*
- IV. *Some further Observations concerning Electricity, by J. T. Defaguliers, LL. D. F. R. S.*
- V. *Extract of a Letter from the Hon^{ble} Edward Legge, Esq; F. R. S. Captain of his MAJESTY'S Ship the Severn, containing an Observation of the Eclipse of the Moon, Dec. 21. 1740. at the Island of St. Catharine on the Coast of Brasil; communicated to the Royal Society by the Rev^d Jos. Atwell, D.D. F.R.S.*
- VI. *An Observation of extraordinary Warmth of the Air in January 1741-2. communicated in a Letter from the Rev. Mr. H. Miles to Mr. J. Eames, F.R.S.*
- VII. *The Description and Uses of the Steel-yard Balance Swing, invented and made by Mr. Timothy Sheldrake.*
- VIII. *A*

The CONTENTS.

- VIII. *A Letter from Sir Tho. Mostyn, Bart. to Roger Jones, M. D. F. R. S. concerning a golden Torques found in England.*
- IX. *A Letter from Benj. Cooke, F. R. S. to Peter Collinson, F. R. S. giving an Account of the Fire-ball seen Dec. 11. 1741.*
- X. *An Account, by Mr. John Eames, F. R. S. of a Book intituled, Jacobi Theodori Klein Historiæ Piscium Naturalis promovendæ Missus primus Gedani, 1740. 4^{to}. Or, The first Number of An Essay towards promoting the Natural History of Fishes; by Mr. Klein, Secretary of Dantzick, and F. R. S.*
- XI. *A Journal of the Shocks of Earthquakes felt near Newbury in New-England, from the Year 1727. to the Year 1741. communicated in a Letter from the Rev^d Mr. Matthias Plant, to the Rev^d Dr. Bearcroft.*
- XII. *An Account of Mr. Sutton's Invention and Method of Changing the Air in the Hold, and other close Parts of a Ship; communicated to the Royal Society by Richard Mead, M. D. Physician to His MAJESTY, F. R. S. and Coll. Med. Lond. Soc.*
- XIII. *A Representation of the Parhelia seen in Kent, Dec. 19. 1741. communicated in a Letter from the Rev^d Mr. H. Miles, to John Eames, F. R. S. and an Account of the same, as seen by Mrs. Tenison at Canterbury.*
- XIV. *EXPERIMENTS, by way of Analysis, upon the Water of the Dead Sea; upon the Hot Spring near Tiberiades; and upon the Hammam Pharoan Water; by Charles Perry, M. D.*
- XV. *An Account of the Case of William Payne, and what appeared upon examining his Kidneys and Bladder when his Body was opened; by Mr. George Bell, Surgeon.*

I. *Extract of a Letter from Mr. Christopher Mason, concerning a Fire-ball seen in the Air, and a great Explosion heard, Dec. 11. 1741.*

*Read at a
Meeting of the
Royal Society,
on January 7.
1741-2.*

ON the 11th Instant, at *Bucksteep* in *Warbleton* Parish, in the County of *Sussex*, about a Quarter before One o'Clock in the Afternoon, I observed a very dark uncommon Appearance in the North, and at the same time the Sun shone bright at my Back; when, on a sudden, there was an Explosion, as violent as the Report of a Mortar-piece, attended with a rumbling Echo, which run Eastward; and as near as I could conjecture, it came from about 40 Degrees of Elevation. Several People saw a Ball of Fire, which ran nearly Eastward, leaving a Train of Light, which continued some time. The Ball of Fire was seen, and the Report heard very loud, at *Sompting*, beyond *Shoreham*. Although I had been gazing upon the black Cloud for some Minutes, yet I saw no Fire nor Lightning. I am, &c.

Dec. 28. 1741.

Chr. Mason.

II. *A Letter from Edward Milward, M. D. to Martin Folkes, Esq; President of the ROYAL SOCIETY, concerning an Antidote to the Indian Poison in the West-Indies.*

S I R,

Read Jan. 7.
1741-2.

AS the ROYAL SOCIETY, over which you so worthily preside, was instituted for the Advancement and Propagation of Natural Knowledge, and as I am convinced, that every the least Step towards such a Design cannot but be agreeable; I shall do myself the Pleasure of communicating to you, an extraordinary specific Antidote against the *Indian* or *Negro* Poison; which, I doubt not, will be the more acceptable, as this Poison hath hitherto been esteem'd the most destructive of any.

The Knowledge of this Remedy was first purchas'd from a famous *Negro* Poisoner, at a great Expence, by one who styles himself, *Isaiah Burges*, *Doctor of Physic*; and the Secret devolv'd to myself, by means of a Manuscript of the Doctor's, which, amongst others, I have procur'd, for my *History of the Physical and Chirurgical Writers of this Kingdom*. The Author intended this little Tract, which contains *Observations on the most considerable Distempers in America*, should be made public; he wrote it, at the Request of his Friends, about when an Expedition was design'd into *America*; and particularly declares, that he purpos'd

pos'd the Divulcation of this specific Antidote, *that such as should go to the West-Indies, amongst the Spaniards, might meet with a Remedy in case of Necessity.* What prevented the Doctor from executing this his laudable Design, I know not; but as it was plainly his Intention it should be made public, and as the Knowledge of such a Remedy may be of the greatest Benefit to Mankind, I shall lay it before you, without any farther Preamble; that it may, by means of your *Transactions*, be communicated to the World, provided you shall judge it worthy of so distinguishing an Honour.

“ The *Negroes*, says he, use a Poison of a strange
 “ and extraordinary Nature. The Dose is very
 “ small, and it hath no ill Taste; so that, mixt with
 “ Meat or Drink, it is not perceivable. It causeth
 “ divers Symptoms, and the Effect is various, accord-
 “ ing as the Dose is large or small. It kills some-
 “ times in very few Hours, sometimes in some
 “ Months, and at others in some Years. The Sym-
 “ ptoms are according to the Quantity given: If
 “ great, it causeth Evacuations upwards and down-
 “ wards; of Excrements first, then of Humours, and
 “ lastly of Blood, with Fainting-Fits, and Sweatings.
 “ Death follows in six or seven Hours. The *Ne-
 “ groes* turn white.

“ If the Dose is but small, the Sick looseth his Ap-
 “ petite, feels Pains in his Head, Arms and Limbs,
 “ a Weariness all over, Soreness in his Breast, and Diffi-
 “ culty of Breathing, (so that one appears as being
 “ in a Consumption) and at last dies languishing.

“ All Remedies yet publicly known, are of no
 “ Force nor Virtue against this Poison; and the Pa-

“tient certainly dies. Nay, I question whether the
 “best Cordial Remedies can put the least Stop to
 “the Efficacy of its Venom, or retard Death, and
 “put it off, longer than the Intention of the
 “cunning Poisoner had fixed it, in proportioning
 “the Dose.

“I know that the *Spaniards* have Knowledge of
 “this very Poison, and am satisfied, that I have seen
 “several *Bocaneers* die of it, given them by *Spanish*
 “Women. I am also persuaded, that it is the same
 “Poison used in *Spain* and *Italy*.

“This Poison hath but one specific Antidote
 “yet known; the Knowledge of which cost me very
 “dear: And it was with much Difficulty I could
 “persuade a famous *Negro* Poisoner to part with
 “his Secret.

“The Antidote is, the Root of the *Sensible Weed*,
 “as it is commonly called, or *Herba Sensitiva*. It
 “grows like a Shrub, has no Prickles, blossoms
 “yellow, and bears little Cods, full of small black
 “pretty Seeds, of which the Women make Neck-
 “laces and Bracelets. Take none of the Root but
 “what is in the Ground; wash it well, and split it
 “in two. Take a good Handful of these Roots
 “so split, and steep them in three Quarts of good
 “clear Water in an earthen glazed Pot, having a
 “Cover. Use but a moderate Fire, that it may boil
 “but very gently. The Decoction has no ill Taste,
 “and you may either give it so, or add Sugar, as
 “you shall think best. Give to the Patient a good
 “Glas of this Decoction, as warm as he can drink
 “it; an Hour after give another, and so for some
 “time, as you shall think it necessary to make a
 “perfect

“ perfect Cure. There is no Danger of giving too
 “ much ; it can do no Harm at all. Several People
 “ have taken this Decoction, though they have not
 “ been poisoned, thinking it would do them good
 “ in other Distempers ; so that one who any ways
 “ suspects he has had some of that Poison given
 “ him, may drink it very safely, and in what Quan-
 “ tity he pleases. The rest of the Plant is to be
 “ rejected as bad and noxious.”

The Doctor enforces his Observations by remark-
 ing, that he had been a Practitioner in those Parts
 for above Five-and-twenty Years. Many *Negroes*,
 he says, were wonderfully preserved and cured by
 taking of this Antidote, though, for Brevity's sake,
 he gives but one Instance ; which is, “ of a strong
 “ *Negro* Man, about thirty Years of Age, and in
 “ perfect Health, who being one Night at a Planta-
 “ tion four Miles distant from that where he lived,
 “ was invited to drink a Dram of Rum, by another
 “ *Negro*, who mixt Poison with it. The Fellow
 “ drank it up, perceiving nothing to be in it ; but
 “ as he was taking Leave, on the other's bidding
 “ him Farewel, and telling him he should never see
 “ him again, he suspected he was poison'd ; and put-
 “ ting his Finger in his Mouth, vomited up great
 “ part of the Poison, though there remain'd enough
 “ of it to cause continual Evacuations in him up-
 “ wards and downwards ; of Excrements first, then
 “ of Humours, and lastly of Blood. As he was
 “ coming home, he fainted away several times, and
 “ calling at length to some Neighbour's *Negro*
 “ Houses, was brought home extremely alter'd ; turn'd
 “ white, and was, as it was thought, expiring. The
 “ Root.

“ Root was immediately sent for, and the Decoction
 “ made, and given him in great Quantity. He con-
 “ tinued taking it for three or four Days, and on
 “ the Fifth went to work along with the rest of the
 “ *Negroes*.”

That the *Sensible Plant* is endow'd with the Property of resisting Poison, hath been, before this, taken Notice of. For the Honourable Sir *Hans Sloane*, the late worthy President of this SOCIETY, whose Writings will always remain an Honour to his Country, hath observ'd from *Piso*, that the *Root* of this Shrub is an Antidote against the *Shrub* itself, which is very poisonous, and kills by Degrees, making the unhappy Sufferers Cachectical, Short-winded, and Melancholy, till they die *. This greatly corroborates what our Author has advanced; and it is observeable, that he likewise directs all Parts of the Plant, except that Part of the Root which is in the Ground, *to be rejected, as bad and noxious*: Though whether this be exactly the same Plant with what our Author mentions, I dare not determine; as Sir *Hans Sloane* inquires whether it be not the *Æschynomene, seu Mimosa arborescens Americana, &c. flore albo*; whereas Dr. *Burgess* expressly says, that it *flowers yellow*: though this may, possibly, be a Mistake in him.

I am sensible it may be objected, that the *Negro* Poison is of various Kinds; and that therefore, though this Remedy may be so extraordinary a Specific in some Cases, it may be unavailable in others. That

* *Nat. Hist. of Jamaica*, Vol. II. p. 57.

the *Negroes* may have the Knowledge of different Sorts of Poison, I deny not; but it would appear, from the Universality of the Effects of this Medicine, as the Doctor affirms *many have been wonderfully cured and preserved by it*, and does not mention a single Instance of its Miscarriage, as though the *Negroes* in the *West-Indies* used but *one* Kind of Poison, or, if different, yet such as comes within the Power of this Remedy. Besides, as we cannot be assured, but by the Consequence, whether the Poison be of that Sort, as to be within the Reach of this Remedy, or not, I think there is all the Reason in the World it should be administer'd under any Suspicion of the *Indian* Poison: Especially, as the Doctor assures us of its great Innocence; and I believe every one will readily agree with me, that it is no small Recommendation of a Medicine, *That let what will become of its good Effects, it can do no Harm.*

Dioscorides hath rightly observ'd in his *Alexipharmics*, *that very different Poisons produce the same Effects upon Human Bodies; and that therefore they are, for the most part, curable by the same Remedies. For though the Kinds of Poisons are various, yet the Effects which arise from them are common, and but few*†. And that Ornament of his Profession, the learned and beneficent Dr. *Mead*, who hath given us a more rational Account

† Fere enim plurimorum Venenorum facultates in consimiles effectus Corpus tandem deducunt: quam ob causam plerisque communia conducunt Auxilia. Varia equidem sunt Venenorum genera: communes tamen, nec ita multi, qui ex iis oriuntur, affectus.

of the Nature and Properties of *Poisons*, and taught us to reason with greater Certainty about such abstruse Points, than any that ever came before him, is of Opinion, *that though there be a great Variety of internal Poisons, as well Mineral as Vegetable, yet they do all of them seem to agree in their primary Effects, and manner of Operation.* Ess. III. And in another Place, *That virulent Plants, although they may be distinguish'd even from one another by particular Virtues, do however kill by a like Operation and Force.* From whence it seems reasonable to infer, that although Poisons may be various in themselves, yet it is not impossible they may be cured by the same Remedy; as they produce like Effects, and seem to kill by a like manner of Operation. And a very remarkable Instance of this we have in all corrosive Poisons, whether of the Mineral or Vegetable Kingdom, which, however different in themselves, produce their Effects universally by eroding the Coats of the *Stomach*, and the *Primæ Viæ*; and which are all curable in like manner, by sheathing and blunting their acrimonious Particles, by means of smooth, lubricating and oleaginous Medicines.

But be this as it will, I think the Remedy deserves, at least, a fair and impartial Trial, as the Author has not indulged in any rhetorical Flourishes, or Theory, but seemingly confin'd himself to Truth, and plain matter of Fact. And, indeed, should it be found to succeed but *One* time in *Twenty*, in such deplorable Circumstances, it cannot but be a Discovery of the greatest Consequence; especially as we are sufficiently assur'd beforehand, that all Cordial and
Alexi-

Alexipharmic Medicines besides, can be of no Service at all. And this may serve as another Argument, why, under any Probability of a Person's being injur'd by the *Indian* or *Negro* Poison, this Remedy should be administer'd; even though we cannot positively be assur'd, whether it be by this very Poison or not: For in Cases where all other Remedies are likely, if not sure, to prove unavailable, we may as well advise this as any.

I cannot affirm, I ever knew any Effects from this Remedy on my own Knowledge or Observation; having never resided in those Places where this Poison is frequent: But surely a Medicine which promises such great things for the Good of Mankind, deserves to have a fair and candid *Examination*; and should it prove to be as serviceable as the Author assures us it is, you cannot but receive the highest Satisfaction, in being instrumental in divulging the Knowledge of a Remedy, from whence the Public may receive such unspeakable Benefit. And it would be doing the highest Service to their Fellow-Creatures, if such as have a proper Opportunity in the *West-Indies*, would administer it, according to the Directions here given, and report an exact Account of its Success.

One thing more I must beg leave to add, with regard to the Trial of this Medicine; that it would be necessary to observe, whether the same Root, dried, would be of equal, or any Efficacy; that if so, the Benefits of it may be extended to other Climates wherein this Herb does not naturally grow: Particularly as the Author is convinced, that the same sort of Poison is used both in *Spain* and *Italy*.

I shall do myself the Honour, some other Opportunity, of laying before you the Conjectures of Authors concerning this *Indian* or *Negro* Poison, with the Remarks I have made thereon; and if the present Paper proves acceptable, shall communicate to you the Doctor's other Observations concerning the *Bites of poisonous Snakes, poisonous Fish, Wounds, and the most remarkable Distempers in the West-Indies*; which cannot but be of great Service to such as reside in those Parts; especially as he mentions chiefly such Medicines as are *Natives* of the Country he treats of, and which are therefore the more easy to be procur'd.

I am, Sir, (with the greatest Esteem)

*Your, and the SOCIETY's,
most Obedient,
and very humble Servant,*

Edward Milward.

III. *A Letter from Edward Nourse, F. R. S. Surgeon to St. Bartholomew's Hospital, to the President and Fellows of the ROYAL SOCIETY, giving an Account of several Stones found in Bags formed by a protrusion of the Coats of the Bladder, as appeared upon opening the Body of one Mr. Gardiner.*

Read Jan. 7.
1741-2.

PErmit me to lay before you the Bladder of Mr. *Gardiner*, who was, the 5th of *March* 1739. before the Trustees appointed by the Parliament 'to inquire into the Efficacy of Mrs. *Stephens's* Medicines, produced as an Instance, where they had been effectual in dissolving the Stone in the Bladder.

Mr. *Gardiner* was searched by me on *Saturday* the 30th of *December* 1738. I felt a Stone the Moment my Instrument was introduced; which was likewise felt by Mr. *Wall*, his Apothecary, then present.

The *Tuesday* following, he began to take Mrs. *Stephens's* Medicines, and continued them eight Months.

On the 30th of *November* 1739. I saw him at *Child's* Coffee-house, when he told me, he was quite free from his usual Disorders: I there searched him again, in the Presence of several Physicians and Surgeons, who likewise felt for the Stone, but none could be found.

Mr. *Gardiner* dying on *Saturday* the 2d of *January* 1741-2. the next Morning, in the Presence of Mr. *St. Hill*, and Mr. *Wall*, I opened his Bladder, and therein observed six preternatural Apertures of different Sizes, the biggest capable of admitting the Top of my Finger. Each of these Openings led to a separate Bag, formed by an Inlargement of the internal Membrane of the Bladder, protruded between the Fibres of its muscular Coat.

These Bags are to be seen on the back Part of the Bladder, a little above the *Vesiculæ Seminales*; and when viewed on the Outside, seem to be but Two; though they are in Number equal to the Openings within, already mentioned; and divided from one another by the Duplication of the internal Membrane, which forms a *Septum* between each of them.

In these *Sacculi*, or Bags, are contained nine Stones; the largest about the Size of a small Nutmeg; and with what Facility some of them moved out of, and returned into, the *Sacculi*, the following Circumstance will clearly evince.

When I had open'd the *Abdomen*, Mr. *St. Hill*, handling the Bladder, brought two of these Stones up to its *Fundus*, where they were felt by Mr. *Wall* and myself.— We then examined the Kidneys: The Right contained a little Matter, otherwise it was as it should be: But of the Left, Two-thirds were wasted; its *Pelvis* was contracted in Proportion, and the *Ureter* almost impervious.— Upon rehandling the Bladder, neither of us could feel any Stone; I therefore laid it open, and we found them all in the *Sacculi*. The Stones that are in one of these

these *Sacculi*, have been so much enlarged since their Lodgment, that without Force and Laceration they cannot be got out.

I am,

GENTLEMEN,

Your most obedient,

Jan. 7. 1741-2.

and most humble Servant,

Edward Nourse.

FIGURE *the First*, (See TAB.)

Shews the Bladder cut open.

1. 2. 3. 4. 5. 6. The preternatural Apertures opening into so many *Sacculi*, in which the Stones were contained.
7. 8. The two *Ureters*.
9. 10. Their Openings into the Bladder.
11. The Opening from the Bladder into the *Urethra*.
12. The prostrate Gland, which was scirrhus and enlarged.
13. The *Urethra* cut off.

FIGURE *the Second*,

Shews the Back-part of the Bladder, upon which the external Membrane being taken away, the Fibres of its muscular Coat are very apparent.

A. The Fibres of the *Detrusor Urinae*.

B. B.

B.B. The *Sacculi* formed by the internal Membrane, protruded between the Fibres of the *Detrusor Urinæ*.

CCCCCCCCC. The Stones, as they appear in the *Sacculi*, Eight in one, and One (the largest N° 6.) in the other.

DD. The *Ureters*.

EE. The *Vesiculæ Seminales* turned back, to shew the whole Extent of the *Sacculi*.

FF. The *Vasa Deferentia*.

G. The Back-part of the prostrate Gland.

1. 2. 3. 4. 5. 6. The Stones which came easily out of the *Sacculi*.

7. One of the Stones sawed, the *Nucleus* of which appears white, and the Surface of them all appears reddish.

IV. Some further Observations concerning Electricity, by J. T. Desaguliers, LL. D. F. R. S.

Dec. 14. 1741.

Read Jan. 14.
1741-2.

ELECTRICS *per se* (which I have heretofore defin'd, *Bodies in which an Electrical Virtue may be raised by some Action on them*, such as Rubbing, Patting, Warming, &c.) are reduc'd to a Non-electric State by being in Contact with Non-electric Bodies, especially Water, which is the greatest Non-electric, even when it becomes Vapour.

A Non-electric (which though it cannot be made Electrical by any Action upon it) receives Electricity from

from an excited Electrical Body; but does not retain it whilst it touches any other Non-electrical Body. An Electric *per se*, when it is become Non-electrical, differs from the Non-electric *per se* in this; that it may be so restor'd to Electricity, by applying a rubb'd Tube to it, as to repel all other Electrics of the same kind of Electricity as the Tube; till it meets with some Non-electric Body, which brings it back to Non-electricity, or at least to such a languid State, that its Electricity is scarce perceptible.

The Electricity may be also restor'd in the same manner by Wax, &c. But in both Cases, an Electric Body, in a languid State, cannot be restor'd to Electricity whilst it adheres to a Non-electric *per se*.

EXPERIMENTS *to illustrate these Assertions.*

From an horizontal Cat-gut (which is an Electric *per se*, as most Animal Substances are) I suspended two Feathers, the one by a Thread, and the other by a Silk, about two Foot long each: Then applying the rubb'd Tube to the Feather hanging by the Silk, (which Silk is an Electric *per se*) the Feather came to the Tube, and stuck to it, as all Non-electric Bodies do, till it was so impregnated with the Virtue from the Tube, as to come out of its languid State, and become strongly Electrical; which appear'd by its flying from the Tube, and being repell'd as often as the Tube was brought near it; till it had touch'd some Non-electric Body, or was left so long as to imbibe the moist Particles floating in the Air; by which it became Non-electric, and was again attracted by the Tube.

When

When I apply'd the Tube to the other Feather that hung by the Thread, (which, like most vegetable Substances, is generally Non electric *per se*) the Feather was constantly attracted, and never repell'd; because the Virtue communicated from the Tube to the Feather, lost itself along the Thread; which would have been retain'd by the Feather, if it had floated in dry Air, or been suspended by an Electrical Body.

These Properties of Electric Bodies shew the Reason of that *Phænomenon*, whereby a rubb'd Tube, after having attracted a Feather, repels and chases it about a Room in the Air, and does not attract it a second time, till the Feather has touch'd some other Body; and also shews the Reason why the Experiment does not succeed in moist Weather.

Pure Air, that is dry, may be rank'd among the Electrics *per se*, because it repels all Bodies in a State of Electricity, whether they have been excited to it by Wax or Glafs; that is, by either of the two sorts of Electricity.

Watery Vapours, that float in the Air, are Non-electric; from which Mixture the Air becomes more languid in its Electricity, when most impregnated with Vapours; so that dry Air is more Electric than moist; but cold Air in frosty Weather, when Vapours rise least of all, is more electric than Air in Summer, when the Heat raises Vapours; which renders that State of the Air more fit for making Electrical Experiments.

The rubb'd Tube retains its Electricity a long time, because it repels, and is repell'd by, the dry Air; and the Feather, which has been attracted by the Tube, after adhering to it a while, is rais'd out of its languid
State

State to a strong Electricity; whereby it flies from the Tube, repels and is repell'd by the Air, where meeting with very few Vapours, it retains its Electricity a long time; till touching a Non-electric, that is brought to it, it loses its own Electricity by communicating it, becomes a Non-electric, and is re-attracted by the Tube, to which adhering some time, it receives so much Virtue from the Tube, as to be restor'd to its Electricity, and again repell'd.

In a moist State of the Air, the Feather, after it has been made electrical, and repell'd by the Tube, it attracts to it the moist Vapours floating in the Air; whereby losing its Electricity, it is attracted by the Tube, without touching any other Body first.

Sometimes, when the Feather flies off from one Part of the Tube, it immediately returns to another Part, generally the Top of the Tube, because the Top of the Tube has attracted the moist Vapours, and is become a Non-electric, and therefore attracts the Feather; which being become electric, flew off from the electric Part of the Tube.

That this is true, appears from an Experiment to be made in dry Weather.

At that Time, when every Part of the Tube repels the Feather strongly, after having attracted it, if you wet two or three Inches of the upper End of the Tube, the Feather will come to that End.

Wetting the Silk by which the Feather hangs from the Cat-gut, the Feather will be always attracted, and not repell'd.

When the Silk is dry, the Feather once made electrical, so as to be repell'd by the Tube, retain'd that Virtue above two Hours in frosty Weather ; but in moist Weather lost it in half a Minute.

V. *Extract of a Letter from the Hon^{ble} Edward Legge, Esq; F. R. S. Captain of his MAJESTY'S Ship the Severn, containing an Observation of the Eclipse of the Moon, Dec. 21. 1740. at the Island of St. Catharine on the Coast of Brasil; communicated to the Royal Society by the Rev^d Jos. Atwell, D. D. F. R. S.*

Dec. 21. 1740.

Read Jan. 21. 1741-2. “ **I** Observed an Eclipse of the Moon, which began very nearly at five
 “ Minutes after Seven ; but the Horizon being hazy,
 “ I could not observe exactly the Beginning : How-
 “ ever, it ended exactly to a Moment at 50 Mi-
 “ nutes after Nine. I set my Watch by two Ob-
 “ servations before, that I might be exact in Time,
 “ and confirm'd it by one after ; so that I believe I
 “ may venture to say it was right : And I observed
 “ with one Telescope on board, and sent another on
 “ Shore, which agreed exactly together.”

This Eclipse was observed at the Island of *St. Catharine*, on the Coasts of *Brasil* ; and the Captain places the said Island in Latitude $27^{\circ} 30'$. Mr. *Gael Mor-*

Morris has calculated the said Eclipse; and the Middle of it, apparent Time, at *Greenwich*, was,

	h.	'	"
	11.	44.	50.
By the Captain's Observation, sup-posing the Beginning exact, . . }	8.	27.	30.

Difference of Meridian . . .	3.	17.	20.
	= 49°	20'	

The End of it, by Calculation } at <i>Greenwich</i> }	h.	'	"
	13.	06.	57.
—by Capt. <i>Legge's</i> Observation	9.	50.	00.

Difference of Meridian	3.	16.	57.
	= 49°	14'	

Capt. *Legge* observes, that in attempting to pass *Cape Horn*, they thought themselves to have been more to the Westward than they really were: By which Mistake, turning too soon to the North, they fell in with high Lands, and met with those Misfortunes, which, if they had kept out more at Sea, might probably have been avoided. By comparing the Longitude at *St. Catharine's* as above settled, with *Senex's* Maps, the Coasts appear to be placed about 6 Degrees too much Eastward; and if the other Parts of *America* about the Cape are laid down as faultily in the Charts, this Error will probably account for their Misfortunes.

Jan. 16. 1741.

Jo. Atwell.

VI. *An Observation of extraordinary Warmth of the Air in January 1741-2. communicated in a Letter from the Rev^d Mr. H. Miles to Mr. John Eames, F. R. S.*

Tooting, Surry, Jan. 20. 1741-2.
Read Jan. 21. 1741-2. **M**Y Mercurial Thermometer abroad, was last Night, at 10 o'Clock, 20 Degrees above the freezing Point; which is higher than it was sixteen Mornings of the one-and-thirty in *May* last, and higher than in any Morning in *April*, one excepted.

VII. *The Description and Uses of the Steel-yard Balance Swing, invented and made by Mr. Timothy Sheldrake.*

Read Jan. 21. 1741-2. **A**S a beautiful and regular Form of Body renders a Person agreeable; so, on the contrary, Deformity of Body not only produces Weakness, but sometimes is the Cause of Ridicule amongst such unthinking People as will not remember, *That it is He that made us, and not we ourselves.*

For the foregoing Reasons, and to prevent such bad Consequences as the above-mentioned, it would be much to the Advantage of crooked Persons, if any Method could be found for giving them any Help, by endeavouring to regain the original Symmetry of Parts, which, by its Commonness, is not sufficiently esteem'd,

esteem'd, though justly valued by such as Crookedness has unhappily deprived thereof.

Where Crookedness is caused by bad Accidents, as Falls, breaking of Bones, or any such Causes, attended with Neglect; there it is to be feared no Help can be given. But where a Deformity of Body is owing to some Defect of Health, ill Habit of Body, or some internal Cause, I hope it is in the Power of Art and Care to prevent growing worse; or with good Care and Endeavours, to recover intirely: For doing which, I hope, this *Steel-yard Swing*, now laid before this Honourable SOCIETY, will be thought an useful Invention for doing such Service to crooked Persons, whose Bones are tender, and capable of having their Form a little alter'd.

The Body, as it is composed of Bones with Joints, cover'd with Muscles, &c. for moving the Body, as Necessity requires, so if any of these Muscles that are of Use for bending the Body forward, backward, downward, or raising it upward, or for turning Part of the Body to the Right or Left Side, have by Illness, want of proper Nourishment flowing so freely to one Side as the other, a careless way of Sitting or Lying, been contracted on one Side of the Body, by which the Bones are braced closer together than Nature intended; in this Case, the Hip generally rises, the Shoulder on the same Side falls lower; the great Support of the Body, the *Vertebræ* of the Back, are alter'd from their natural Uprightness to a Curve, and the other Side extended to too great a Length: Thus the *Viscera* are pressed too close on the contracted Side, and probably hindered from performing their due Office; whilst on the contrary Side, which

which is extended beyond its true Bounds, there is too much Room for them, that may give too large a Growth to them, or render them too lax and weak. From this united ill State of the *Viscera* it is possible that crooked Persons are generally unhealthy.

For removing this distorted Form, and recovering a better, this *Steel-yard Swing* is proposed, as a mechanical Method, for stretching the contracted Side, and giving Liberty to the too-much extended Side to contract; that the Sides may thereby be brought to their original and regular Form, by suspending the crooked Person with Cords properly cover'd for Ease, and put under each Arm, and then placed at equal Distances from the Centre of the Beam. The Gravity of the Body will, in great Probability, immediately affect the contracted Side of the Body, so as to put the Muscles a little upon the Stretch; and if the Cord under the Arm on the longest Side of the Body be remov'd further from the Centre, the longest Side will become a Weight continually increasing, as the Point of Suspension is remov'd further from the Point of Motion; by which means the shortest Side must be lengthen'd. Thus the *Vertebrae* of the Back will be gradually brought from their irregular Form, to a perpendicular; and the Head, that probably lean'd too much to one Side, will rise upright.

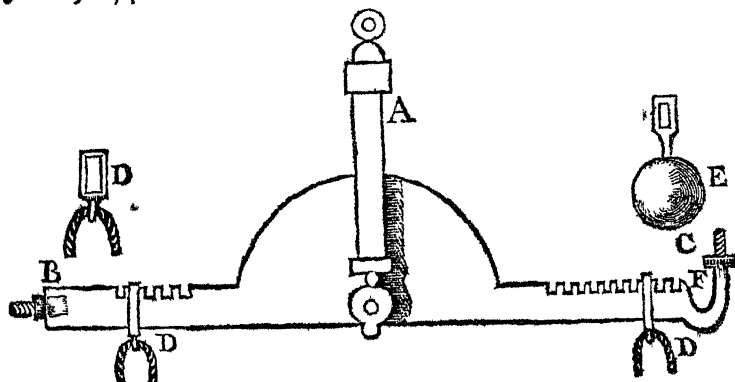
The Child, or crooked Person, may hang suspended much longer upon this Swing, than by the Head in one of the Semicircular Swings, which cannot extend the contracted Side in such manner as this can, as will appear by the just Observation of this Instrument. It may be necessary to keep the Arms
down,

down, by a small Bandage round the Body and Arms a little above the Elbow.

By this Method of swinging a Child, its own Weight must consequently stretch the contracted Muscles, &c. that draw the Shoulder and Hip too close together, and give Liberty to the Ribs to extend themselves to a greater Distance from each other; and at that very Moment of Time, the too-much extended Side, by the Weight of the Body, will be pressed closer together; and by daily increasing the Time that the Person is upon the Swing, the desired Effect may be produced, an agreeable Form of Body recovered, and a healthy Constitution restored, to the Satisfaction of the Parents, and great Benefit of the once crooked Person.

*London, over-against
Cecil-street in the Strand,
Jan. 19. 1741-2.*

Tim. Sheldrake.



ABC, Is the Steel-yard Balance-Swing.

*D, One of the square iron Loops to which the
Cords are to be fix'd, and which Loops, one
ON*

on each Arm of the *Balance*, are moveable from one Notch to another.

E, A Weight, to be hung upon the Arm *C* at *F*, to add to the Weight of the too-much extended Side, as Occasion requires.

VIII. *A Letter from Sir Tho. Mostyn, Bart. to Roger Jones, M. D. F. R. S. concerning a golden Torques found in England.*

Read Jan. 28. 1741-2. *S I R,* *Gloddeth, Dec. 27. 1741.*
I Have received the Favour of your Letter, wherein you desire to be informed of the Particulars of my *Torques*. I wish I were able to give you a better Account, but have never seen nor heard of any Remarks made upon it, or any Account where it was found, but I think it was in this County. It is a Wreath of Gold, weighing, as near as I can judge, nine Ounces. I believe it is without Alloy, being very pliable; it answers exactly *Virgil's* Description, *Æn. V. 558*; and *559.*

*Pars leves humero pharetras: it pectore summo
 Flexilis obtorti per collum circulus auri.*

It being joined here with the *Pharetra*, and being very proper for carrying a Quiver, inclines me to think, that the *Gauls*, from whom the *Romans* took it, used it for that Purpose; but among the latter it seems to have been worn as an Ornament, rather than a thing of Use. There are several Passages in the
 Histo-

Historians, which mention its being given as a Reward for military Service. It is sometimes described as a Chain consisting of several Links; but mine is all one Piece, without any Link or Joints, and takes its Flexibility from the Purity of the Metal.

I doubt not there are many Gentlemen of the SOCIETY, who can give a better Account of the *Torques* than I can. If, for your own Satisfaction, you have a mind to be further informed of the Use of it among the Antients, you may, I believe, find it in a Treatise written by one *John Schefferus, de Antiquorum Torquibus*, which is printed in *Grævius's* Collections, but I have not the Book here.

I have not forgot the Oil or Scum which floats on the Sea about *Midsummer*, and will endeavour to have some saved the next Season.

I am, Sir,

Your most humble Servant,


Tho. Mostyn.

IX. *A Letter from Benj. Cooke, F. R. S. to Peter Collinson, F. R. S. giving an Account of the Fire-ball seen Dec. 11. 1741.*

*Newport, in the Isle of
Wight, Jan. 25. 1741-2.*

Read Jan. 28. 1741-2. **I** Did not see the *Phænomenon* (the *Fire-ball* seen *Dec. 11. 1741.*) you mention *; but a Gentleman of my Acquaintance was

* See in this *Transaction* p. 1.

on an Hill about three Miles West of this Town, and had a very advantageous View of it.—He says, that at that Time the Brightness of the Sun was a little obscured by the Interposition of some thin Clouds, when he saw it pass by to the Eastward, at about the Distance of something more than a Quarter of a Mile, and apparent Height of 30 Feet above the Level of the Place where he stood. Its Colour was that of a burning Coal; its Figure a Cone, whose Length might be eight Feet, and Diameter at the Base 18 Inches. From about its Apex, which was its hinder Part, issued several bright Streams sparkling with fiery Drops, to the Length of about four or five Feet, something after this manner 

—Its Motion was nearly parallel to the Plane of the Horizon, and its Direction (as near as we can find by comparing the Places it passed over) from South-west by South to North-east by North, without any Noise, Wind, or Motion of the Earth attending it.—The Time of its Appearance did not happen to be taken Notice of with the desired Exactness; but by the best Observation we can make, must be about a Quarter before One o'Clock at Noon.—There were a few others who saw it, to whom it appear'd different in Shape, according to the Point it was seen from; and perhaps its Shape might change as it became nearer consuming, and only its Head, in the Form of a Bell, remain at last.—As this was the first Land it happen'd to make on this side the Chanel, I should be glad to know if any Accounts can be got of it Abroad, and what the Learned think of it; and shall think

think it a great Happiness, if this Relation may be of any Use or Satisfaction. I am,

S I R,

*Tours most obliged and
most affectionate,*

B. Cooke.

- X. *An Account, by Mr. John Eames, F. R. S. of a Book intituled, Jacobi Theodori Klein Historiæ Piscium Naturalis promovendæ Missus primus Gedani, 1740. 4^{to}. Or, The first Number of An Essay towards promoting the Natural History of Fishes, by Mr. Klein, Secretary of Dantzick, and F. R. S.*

*Read Feb. 4.
1741-2.*

ALthough the Natural History of Animals has been vastly improved, since several of the worthy Members of the ROYAL SOCIETY, both at Home and Abroad, have taken it under their Consideration; yet there still remain some things to be known, in order to render it full and complete. As particularly, concerning the Hearing of Fishes, it is remarked, that in no Fishes beside the Cetaceous Kind, have hitherto been found any Auditory Passages, or Ear-holes; and whether all Fish hear or no, is a Question not yet fully determined, notwithstanding the Experiments alledged to prove the Affirmative.

'Tis with this View, and in order to set this Matter in a clearer Light, the ingenious Author has obliged the World with the Book before us.

It consists of a Dedication address'd to this Honourable SOCIETY, a Preface, an Essay, and a double Appendix.

The Preface begins with acquainting us, what he means by Fishes, and defines them so, as to exclude several Tribes, that have been commonly taken for such by the Antients. *Pisces dicimus Animalia* (says he) *apoda pinnis natantia*; and adds in his Annotation upon it, *Ab hac definitione seclusa sunt Serpentium Genus, pinnis carens, Cancri, Astaci, Testacea; Cochleæ, Conchæve; imo Amphibia, sive Bipeda, (ut Manati Clusii) sive Quadrupedia, ut Phocæ, &c.*

Mr. Klein then (waving the Consideration of the Cetaceous Kind, which are allow'd by most Authors to have both Auditory Passages, and the Sense of Hearing) proceeds to the main Question, *viz.* Whether the Cartilaginous and Spinose kinds of Fishes are endued with the Sense of Hearing; or have any Organs or Auditory Passages for that Purpose.

He gives us the Sentiments of the Antients and Moderns, by producing a Variety of Quotations, both *pro* and *con.* out of their Works; from whence (says he) it appears, that though some of them were dubious, yet many of them agree, that Fishes do hear; nevertheless, none of them were fully satisfied, by what Part, or Ways, they had this Sensation produced. And though *Julius Casserius Placentinus* found out some little Bones in the Head of the *Pike* or *Jack*, which he look'd upon to be the Organs of Hearing,

Hearing, yet he could not discover any manifest external Auditory Passages.

In fine, from a diligent Inquiry into, and Consideration of all, that hath been said from Reason and Experience on both sides the Question, our curious Author determines us in favour of the Affirmative; and says, That Fishes not only have Organs of Hearing, but also Passages, (though they are difficult in many Species of them to be demonstrated) by means of which a tremulous Motion is communicated to these Organs. Nor does he think the Water in which they live, any Impediment, but rather the Medium, (or, as he calls it, the *Intermedium*) by which Sound is communicated to them: As a Man shut up in one Room, will hear and understand what is said in another, notwithstanding the Interposition of a Party-wall.

Our ingenious Author then proceeds to his Essay, wherein he considers what Parts in the Head of Fish serve for the Organ of Hearing, and by what Passages a tremulous Motion producing this Sensation may arrive at them. This Part of his Treatise he styles, *De Lapillis, eorumque Numero in Craniis Piscium*. These little Bones, sometimes called *Officula*, or little Bones, Mr. *Klein* looks upon, and accordingly considers, as constituent or essential Parts in the Heads of Fish, and generated with the Brain itself. They differ (he says) in Magnitude, according to the different Size or Bulk of the Fish to which they respectively belong, and are easiest to be discover'd in Heads of the Spinose Kind.

There are in all kinds of Fish three Pair of them; the first are the two largest Bones, and are easily
enough

enough found; but the greatest Difficulty lies in discovering the other two Pair, which are small, and lie envelop'd in distinct little Bags, or a fine sort of Membrane. These he takes to be the Auditory Organs, and answer to the *Incus*, *Malleus*, and *Stapes*, in other Animals: And he thinks by a diligent and careful Inspection, we might determine the Age of Fishes, by the Number and Thickness of the *Laminae* and *Fibres* of these Bones, as we can the Age or Growth of a Tree, by the Number of Circles in the woody Part of its Trunk.

The Passages by which a tremulous Motion producing the Sense of Hearing, may arrive at these Auditory Organs, are what our Author next inquires after; and he produces first a Specimen in the Spinose Kind, *viz.* in a *Jack* or *Pike*; and upon Inspection into the Head of this Fish, he observes several Holes, which, by means of Hogs Bristles, he finds lead directly to these Auditory Bones before describ'd.

In dissecting the Head of a *Sturgeon*, (as a Specimen of the Cartilaginous Kind) he traced the Auditory Duct as far as the Membranous Body in which the three Pair of little Bones are placed.

But as our curious Author has oblig'd us with an exact Delineation of these Auditory Ducts or Passages, as well as the Figures of a Variety of those *Lapilli* or *Ossicula*, from different sorts of Fish, on several Copper Plates, to these I must refer, for a more satisfactory Idea than can possibly be given in Words.

We therefore proceed to the first Appendix, which entertains us with the Anatomy of a *Porpest*. This
Fish

Fish our Author in the Title Page styles *Tersio*, the usual Name for it in *Pliny*; but he calls it *Phocæna* in the Appendix, the Name used for it by *Aristotle*.

'Tis ranked amongst the Cetaceous Kind, and is the smallest Fish in that Tribe, seldom exceeding five Feet in Length; in which it differs from *Dolphins*, (amongst which Species it has by some been improperly reckon'd) for they often exceed ten Feet in Length. The Snout also of the *Dolphin* is much larger than in the *Porpeß*, which is another thing sufficient to distinguish them. It would be needless here to give a Detail of the Anatomy of this Animal, which is so largely done by Dr. *Tyson* in his *Phocæna*. I shall therefore only take Notice of some Remarks made by our Author upon the Dissection of a *Porpeß*, by the accurate Hand of Dr. *De la Motte*, at Mr. *Klein's* Request.

In the first Place, the *Meatus Auditorius* was found by both to be two Inches distant from the exterior *Canthus* of the Eye, forming a very small Hole (lest the Water getting in might prove an Inconvenience to it). He then gives us an Account of the *Os Petrosum*, and other Auditory Organs, with curious Figures of them; in order to correct Mr. *Ray*, who in *Philosophical Transactions*, n. 76. p. 2278. says, *We observed not in this Fish any Ear-holes or Meatus Auditorii at all, wherein also Aristotle agreeth with us.*

A second Remark is, that though the *Porpeß* has no *Vesicula fellea* or Gall-bag, (and from thence most Authors have been induced to believe no Gall) yet Dr. *De la Motte*, upon a more exact Scrutiny, finds a Duct that arises with a great Number of Branches
in

in the Liver, and tending downwards, joins itself to the Pancreatic Duct; and these two, so united together, form a Canal or common Duct, about four or five Lines long, before they discharge their Contents into the *Duodenum*. From whence it appears, (says Dr. *De la Motte*) that the *Porpefs* has always a Discharge of Bile into the *Duodenum*, though 'tis but thin and diluted, and such as in other Animals is usually called Hepatic Bile.

In dissecting the *Os Petrosum*, several Worms were found: Some of these Mr. *Klein* has presented us with a Figure of, as also of the Parts of Generation proper to the Male *Porpefs*, and lastly the Thoracic Duct in its natural Dimensions.

Our ingenious Author concludes with some Observations made on the Heads of two *Raiæ* of an uncommon Species, and which he says are no-where described. He gives us the Figures of the Auditory Organs, with the Jaw of one of these Fish very accurately depicted in his vith Table.

And having consider'd the Auditory Organs, with the Seat of them both, in the Cetaceous, Cartilaginous, and Spinose Kinds of Fishes, it appears, says our ingenious Author, that these *Lapilli* or *Ossicula* differ from one another both in Structure and Substance; for in Cetaceous Fishes, whose Skeletons are truly bony, and which, in certain respects, may be compared to truly Lignous Trees, both the *Os Petrosum*, and Auditory Organs, are in these, as in other Animals, perfectly *osseous* or bony: Whereas the Cartilaginous Fish, whose Skeletons are Elastic and Cartilaginous, they may be compared to the *Keratophyta* Species of Sea-Plants; and these Fish, instead

instead of an *Os Petrosum*, have something analogous, but cartilaginous; and the Auditory Bones are of a tartareous kind of friable and easily mace-
rable Substance.

XI. *A Journal of the Shocks of Earthquakes felt near Newbury in New-England, from the Year 1727. to the Year 1741. communicated in a Letter from the Rev^d Mr. Matthias Plant to the Rev^d Dr. Bearcroft.*

S I R,

Read Feb. 11.
1741-2.

IT may be acceptable, if I give an Account of the Earthquake, as I took it down precisely at every time I heard it.

Oct. 29. 1727. being the Lord's-Day, about 40 Minutes past Ten the same Evening, there came a great rumbling Noise; but before the Noise was heard, or Shock perceived, our Bricks upon the Hearth rose up about three quarters of a Foot, and seem'd to fall down and loll the other way, which was in half a Minute attended with the Noise or Burst. The Tops of our Chimneys, Stone-fences, were thrown down; and in some Places (in the lower Grounds, about three Miles from my House, where I dwell) the Earth opened, and threw out some Hundred loads of Earth, of a different Colour from that near the Surface, something darker than your white Marl in *England*; and in many Places, opened dry Land into good Springs, which remain to this Day; and dried up Springs, which never came again.

E

It

It continued roaring, bursting, and shocking our Houses all that Night. Though the first was much the loudest and most terrible, yet eight more, that came that Night, were loud, and roared like a Cannon at a Distance. It continued roaring and bursting 12 times in a Day and Night, until *Thursday* in the said Week, and then was not so frequent; but upon *Friday* in the Evening, and about Midnight, and about Break of Day upon *Saturday*, three very loud Roarings: We had the roaring Noise upon *Saturday*, *Sunday*, *Monday*, about 10 in the Morning, though much abated in the Noise.

Nov. 7. being *Tuesday*, about 11, it roared very loud, and gave our Houses a great Shock; and continued shocking from three times to six every Day and Night until the 12th of *November*, when it was heard twice in one Hour in the Afternoon, from half an Hour after Three to half an Hour after Four. Sometimes the Roaring of the Earthquake was loud, other times it seem'd at a Distance, and much abated. Upon the 13th of *November*, being *Monday*, two Hours before Day-break, the Roaring was loud, and shook the Houses. Upon *Wednesday* following, half an Hour past Two in the Afternoon, there was a Roaring, but not so loud. It continued sometimes roaring loud, and shocking our Houses, for five, six, to ten times a Week, until the 17th of *December* following; and then about half an Hour past 10 in the Evening, being *Sunday*, it roared very loud, and shook our Houses very much; another Shock the next Morning about Four, much abated.

January 3. 1727-8. about Nine at Night, an easy Shock.

Jan.

Jan. 6. Saturday, there were five Shocks, attended with the Roaring, from about Nine at Night to Four on *Sunday* Morning, which I heard; and some People told me, who lived in the low Grounds, that for the Space of about half an Hour, it continually kept roaring every half Minute or Minute.

Upon *Wednesday Jan. 24.* about half an Hour after Nine at Night, it roared exceeding loud, and was followed in half a Minute with Roaring much abated in the Noise.

Jan. 28. Sunday, about half an Hour after Six in the Morning, another easy Shock, and another about Ten the same Morning, easy: On the same Night about one, a loud Roaring and Shock.

Jan. 29. I heard it twice, though easy, that Day.

Tuesday Jan. 30. About Two in the Afternoon, there was a very great Roaring, equal to any but the first, for Terror: It shook our Houses so, as that many People were afraid of their falling down; Pewter, &c. was shook off our Dressers; the People that were in the Church for Evening Service, ran out; the lead Windows rattled to such a Degree, as that I thought they would all be broke. And there was another Shock the same Day, about an Hour and half after, though much abated.

Feb. 21. About half an Hour past 12 at Midnight, a considerable loud Roaring with a Shock.

Feb. 29. Such another.

March 17. About Three in the Morning, *Sunday*, an easy Shock.

March 19. Forty Minutes past One at Noon, a small Noise; at Nine the same Night, a small Noise with a Shock.

April 28. 1728. Sunday, about Five in the Afternoon, a small Noise, but perceivable.

May 12. Sunday Morning, about 40 Minutes past Nine, a long and loud Roaring, and shook the Houses.

May 17. Friday, about Eight in the Evening, a long and loud Roaring shook our Houses.

May 22. Wednesday, several small Roarings in the Morning; but about Ten the same Morning, long and loud, and shook our Houses.

May 24. Friday, about Eleven at Night, loud and long Roaring, shook our Houses.

Thursday June 6. Saturday 8. about Three in each Morning, a long and loud Roaring.

Tuesday June 11. Nine in the Morning, a small Noise.

July 3. about Two in the Morning, and *July 23. Monday* Morning, about Break of Day, very loud and long, shook our Houses.—— Besides these Times I have mentioned, it has been often heard by me; but the Noise was small, so forbore to set them down: I had Thoughts to have added no more Account of the Noise and Repetition of the Earthquake in my Church-book; but acquainting my People with what I had done, they prevailed upon me to proceed again, which I did upon *March 19. 1728-9. Wednesday*, betwixt Two and Three in the Afternoon, it was loud and long, shook our Houses, being repeated twice in an Instant; and this was the longest and loudest Roaring, and the greatest Shock, that I ever heard, the first excepted, and that upon the 30th of *January*, mentioned before. We had several small Shocks in this Interim.——But upon
Sept.

Sept. 8. 1729. Monday, about half an Hour past Three, it was loud and long.

Sept. 29. Monday, about half an Hour past Four in the Afternoon, loud and long.

Oct. 29. I heard it twice this Night; one of the times was about the same time of Night the first Shock was.

Nov. 14. about Eight in the Morning, loud and long, attended with two Bursts like unto two sudden Claps of Thunder; shook our Houses.

Nov. 27. about Eight in the Evening, a very great Roaring, and a great Shock: It was heard at *Ipswich*, about 14 Miles distant.

February 8. 1729-30. about Eight in the Evening, a small Shock, about Midnight loud and long, and gave our Houses a great Shock.

Feb. 26. Thursday, about a quarter before Two in the Morning, the Noise was repeated twice in about one Minute: The first was loud and long, and shook our Houses equal to any but the first Shock; the second Noise was low, and seemingly at a Distance.

April 12. 1730. About Eight in the Evening, *Sunday*, a very loud and long Noise, and a great Shock, equal, I thought, to any for Length and Noise, the first excepted.

July 28. About Nine in the Morning, a sudden and loud Roaring and Shock.

Aug. 15. About Eight in the Morning, a Shock of the Earthquake, twice repeated in a Moment of Time.

Nov. 6. About 11 at Noon, it was loud and long, and gave my House a Jar.

Nov.

Nov. 14. Saturday, about Nine in the Morning, a small Noise and Rumbling, no Shock.

Nov. 25. About 20 Minutes past Eight at Night, a loud and long Roaring, and gave my House a considerable Shock.

Dec. 6. About a quarter of an Hour before 11 at Night, *Sunday*, it was loud, and roared long, and made our Houses jar.

Dec. 11. Friday, about a quarter before Seven at Night, there was a small Burst, but shook my House.

Saturday Dec. 19. about half an Hour past 10 at Night, the Earthquake did very much shake our Houses, without any Noise or Roaring, more than ever before, the first time excepted. It was felt at *Boston* 40 Miles, at *Piscataqua* 22 Miles, almost equal to what it was with us.

January 7. 1730-1. About Seven at Night it was loud and long, shook our Houses.

Jan. 11. About Midnight, loud and long, shook our Houses.

March 7. Sunday, about Five in the Evening, we heard the Noise, but no Shock.

May 28. 1731. About Nine in the Morning, I heard the Noise of the Earthquake very distinctly, but could not perceive, that it shook.

July 5. Monday Morning about Sun-rise, it was loud and long, shook our Houses.

Aug. 21. Saturday, Nine in the Evening, the Noise was small and short.

Oct. 1. Monday, about 11 at Night, loud and long, shook our Houses.

February 7. 1731-2. About Seven at Night, a great Shock, shook our Houses.

Septem-

September 5. 1732. Tuesday, about Noon, we had a severe Shock, which was perceived at *Boston* and *Piscataqua*, but attended with little or no Noise. The same Earthquake was heard at *Mountreal* in *Canada*, at the same Time, and about the same Hour of the Day, and did Damage to 185 Houses, killed seven Persons, and hurt five others; and it was heard there several times afterwards, only in the Night, as the News Papers gave us this Account.

Dec. 30. In the Morning we had a Shock, and it had been heard by some People several times within three Weeks before.

Thursday March 1. A loud and long Noise of it.

October 19. 1733. A loud and long Noise about Midnight.

January 16. Wednesday, 1733-4. About 20 Minutes past 10 at Night, a loud and long Roaring.

June 29. 1734. About a Quarter past Three in the Afternoon, there was somewhat of a Noise of it.

Oct. 9. About 20 Minutes past 10 in the Morning, a small Shock.

Nov. 11. or 12. for it was about Midnight, we had the loudest Noise, and the greatest Shock, except the first: It was long, very awful and terrible.

Nov. 16. About Six in the Morning, there was a small Shock.

February 2. 1735-6. About a quarter of an Hour before Six in the Evening, there was a pretty loud Noise and Shock.

March 21. About half an Hour past 10 in the Morning, it was somewhat loud.

July 13. 1736. About Three quarters after Nine in the Morning, the Noise of it was loud.

Oct.

Oct. 1. Friday, about half an Hour past One at Midnight, it was loud and long, and a great Shock, twice repeated in an Instant.

Nov. 12. About Two in the Morning, there was a Shock with the Noise, and about Six the same Morning it was something louder.

February 6. 1736 7. About a quarter past Four in the Afternoon, we had a considerable Shock.

September 9. 1737. Friday, about 20 Minutes past 10 in the Morning, it was very loud and long, and shook our Houses very much.

Dec. 7. A little before 11 in the Night, the Ground shook very much, but heard no Noise. Upon the same 7th of *December*, at *New-York*, they had three severe Shocks of an Earthquake in the Night: It threw down there some Chimneys, and made the Bells to toll so as to be heard. At the same time the said Shock and Noise was felt and heard in many other Places.

August 2. 1739. We had a great Shock; it made my House shake much, and the Windows jar. It was about half an Hour past Two in the Morning: I think I never heard but two either louder, or longer, or greater.

Sunday, Dec. 14. 1740. About 35 Minutes after Six in the Morning, there was heard a pretty loud Noise of the Earthquake.

Sunday, Jan. 18. 1740-1. About Four in the Morning, there was heard the Noise of the Earthquake.

Sunday, Jan. 25. 1740-1. About 10 Minutes before Four in the Afternoon, there was a Shock of the Earthquake, with a loud rumbling Noise. This is the last that has been heard, (and I pray God I may never

never hear any more such and so long). I have omitted to set some down that were small, or such as I did not hear myself: I was very exact to the Time, so that what Account I have sent you is most certainly true.

I thought an exact Account of so remarkable a Judgment, continued so long, might be acceptable: And although the first Night was the most terrible, as the Surprise was sudden; yet there never happen'd one Shock amongst us, but what occasion'd some Alteration at that time in every Person's Countenance and Constitution; and which way soever any Person's Face happen'd to be, that way the Noise of the Earthquake appear'd to him: And I have frequently, in my Conversation with sundry Persons, been told by them, that for a few Minutes before a Shock of it came, they could foretel it by an Alteration in their Stomachs; occasion'd (as I suppose) by an Alteration in the Air: I attest to the Truth of the Thing by my own Experience. You'll please, Reverend Doctor, to excuse the Length of it, if not acceptable, from

*Your most obedient and
Humble Servant,*

Matt. Plant.

P O S T S C R I P T.

I forgot to tell you, Sir, that (except the first Shock) these frequent Repetitions of the Roaring and Shocks of the Earthquake were upon *Merrimack River*, and seldom extended above seven or eight Miles Distance from, or 20 or 30 up the said River;

F

those

those Instances only excepted, which I have mention'd in the Relation ; and the first Shock of it was greater with us than any-where else in *New-England* ; and the Tops of Chimneys, and Stone-fences, were thrown down only in these Parts.

XII. *An Account of Mr. Sutton's Invention and Method of Changing the Air in the Hold, and other close Parts of a Ship ; communicated to the Royal Society by Richard Mead, M.D. Physician to His MAJESTY, Fellow of the Royal Society, and of the Royal College of Physicians, LONDON.*

Read Feb. 11.
1741-2.

IT is found by daily Experience, that Air shut up and confined in a close Place, without a Succession and fresh Supply of it, becomes unwholsome, and unfit for the Use of Life.

This is more sensibly so, if any stagnating Water be pent up with it.

But it grows still worse, if such an Air as this is made use of in Respiration, that is, becomes moister and hotter, by passing and repassing through the Lungs.

These bad Effects, in different Degrees, according to the different Manner in which Air is inclosed, are observed in many Cases ; particularly in deep Wells and Caverns of the Earth, in Prisons or close Houses, where People are shut up with Heat and Nastiness : But most of all in large Ships, in which,
with

with the Stench of Water in the Hold, many Men being crouded up in Close-quarters, all the mentioned Circumstances concur in producing greater Mischief than would follow from any of them single.

The Reason of these bad Effects is this: It is that Property of the Air which is called its Elasticity or Springiness, which makes it so useful to our Life. When any Part of it is inclosed and kept from the Communication of the outward Air, it expands itself, and, in Proportion to the Closeness of the Place, loses its Spring; and if any Heat or Moisture comes to it, the elastic Force may be quite lost and destroy'd: And not only so, but if it happens to be impregnated with noxious *Effluvia*, either from unwholesome Substances of any kind, or from the infectious Breath of diseased Bodies; it will become quite poisonous and deadly, in a manner suitable to the original Cause.

It is proposed at present to find out a Remedy for this Evil in Ships only: But by making Alterations according as particular Places require, the same may be applied to any Houses or Parts of them, as Prisons, the sick Wards in Hospitals, &c.

Now it is a natural Consequent of the Elasticity of the Air, that when it is rarefied in any Part, (which is most effectually done by Heat) the neighbouring Air will rush that way, till this Part is brought to be of an equal Density and Elasticity with itself; and this again will be followed by the Air next to it: So that, if a Conveyance for Air be laid from the Hold or Well of the Ship, and a Rarefaction of the Air therein be made; the foul Air from this Place will

run or be drawn out that way, and fresh Air from the adjacent Parts will succeed in its room.

It is upon these Principles that the following Scheme is most humbly offered to the Right Honourable the Lords of the Admiralty, and Commissioners of the Navy, which it is hoped will be found effectual for clearing the bad and corrupted Air from the Holds and other close Parts of his MAJESTY'S Ships; and thereby prove beneficial to the Public, by preserving the Healths of many of his MAJESTY'S good Subjects serving on board the same; the whole thing being indeed easy to be executed, and what will no ways incumber, or be troublesome, in any of the Vessels where it shall happen to be applied; the same being, in short, no more than this: That whereas in every Ship of any Bulk there is already provided a Copper or Boiling-place proportionable to the Size of the Vessel, it is proposed to clear the bad Air by means of the Fire already used under the said Coppers or Boiling-places, for the necessary Uses of the Ship.

It is well known, that under every such Copper or Boiler, there are placed two Holes separated by a Grate; the first of which is for the Fire, and the other for the Ashes falling from the same; and that there is also a Flue from the Fire-place upward, by which the Smoke of the Fire is discharged at some convenient Place of the Ship.

It is also well known, that the Fire once lighted in these Fire-places, is only preserved by the constant Draught of Air through the forementioned two Holes and Flue; and that if the said two Holes are closely stopp'd up, the Fire, though burning ever so briskly before, is immediately put out.

But

But if after the shutting up the above-mention'd Holes, another Hole be opened, communicating with any other Room or airy Place, and with the Fire; it is clear, the said Fire must again be raised and burn as before; there being a like Draught of Air through the same, as there was before the stopping up of the first Holes: This Case differing only from the former in this, that the Air feeding the Fire will now be supplied from another Place.

It is therefore propos'd, that in order to clear the Holds of Ships of the bad Air therein contained, the two Holes above-mentioned, that is, the Fire-place and Ash-place, be both clos'd up with substantial and tight iron Doors; and that a copper or leaden Pipe, of sufficient Size, be laid from the Hold into the Ash-place, for the Draught of Air to come in that way to feed the Fire. And thus it seems plain from what has been already said, that there will be from the Hold a constant Discharge of the Air therein contained; and consequently, that that Air so discharged must be as constantly supplied by fresh Air down the Hatches, or such other Communications as are open into the Hold; whereby the same must be continually freshen'd, and its Air render'd more wholesome, and fit for Respiration.

And if into this principal Pipe so laid into the Hold, other Pipes are let in, communicating respectively either with the Well or lower Decks, it must follow, that Part of the Air consumed in feeding the Fire, must be respectively drawn out of all such Places, to which the Communication shall be so made.

XIII. *A Representation of the Parhelia seen in Kent, Dec. 19. 1741. communicated in a Letter from the Rev^d Mr. H. Miles, to John Eames, F. R. S. and an Account of the same, as seen by Mrs. Tennison at Canterbury.*

S I R,

*Read Feb. 25.
1741-2.* **I** Had Yesterday the Favour of yours of the 23^d Instant, for which I return you my very hearty Thanks; particularly for the Honour you put on my brief Account of the Height of the Liquors in my Thermometers, * in communicating it to the ROYAL SOCIETY.

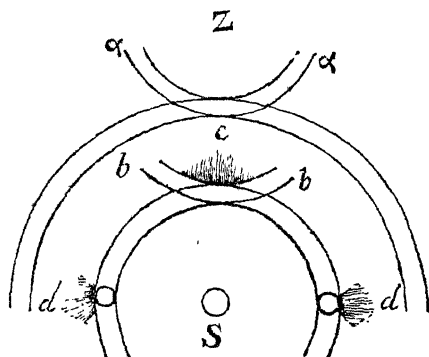
As no one wishes the Prosperity of that illustrious Body more truly than I do, I should think myself happy, were I able to do any thing to promote the Design of it, or to gratify any of its worthy Members.

I have inclos'd a Draught of the *Parhelia* seen in *Kent* the 19th of *December* last, as I took it from a private Letter sent from thence to a Gentleman in this Town: The Writer of the Letter is not so particular in his Account of it as could be wish'd: His Words were to this Purpose: " As to the Appearance
" of the Mock-Suns on the 19th of *December*, I have
" inclos'd a Scheme, such as I could draw, in which
" you may observe *S* is the Sun, *Z* the *Zenith*—*aa*
" an inverted Rainbow of the most lively Colours;
" the Mock-Suns *dd* were sometimes almost too
" bright to look upon, and then they seem'd round,

* See *above*, p. 20.

“ but often were fring’d (as drawn) with the prismatic Colours; the Arch *bb* was but faint, and a whitish Light in the inner Part described at *c*. The Appearance ended about Noon, or rather a little before Twelve; how early the Whole was to be seen, I do not know.”

The two largest Semicircles, I find no Notice taken of. —



On *Tuesday* the 19th Instant, at 10 at Night, being the Time I generally register the Account of the Barometer and Thermometer, I found the Mercurial Thermometer abroad, at 20 Degrees above 0, or freezing Point: This I thought Extraordinary, and for that Reason I consulted my Register of last Year, and found 16 Mornings and 13 Evenings in *May* colder Air. And in *April* there were two Mornings and three Evenings only, a warmer Air. I am

*Your most obliged and
most humble Servant,*

Wind was West all Day. 19th, and began to rise when I made the Observation, at going to Bed.

H. Miles.

‘ Dr.

‘ Dr. *Stukeley* likewise gave in a Scheme of the same Appearance, as it was seen at *Canterbury* 10 h. 12 m. Dec. 19. 1741. in which the Light at *c.* was not taken Notice of. He copied it from a Drawing made by Mrs. *Tennison*, who sent it in a Letter to his Grace the Archbishop of *Canterbury*.’ C. M.

XIV. EXPERIMENTS, *by way of Analysis, upon the Water of the Dead Sea; upon the Hot Spring near Tiberiades; and upon the Hammam Pharoan Water; by Charles Perry, M.D. made on his Journey through the Holy Land, &c.*

EXPERIMENTS (*by way of Analysis*) *upon the Water of Asphaltis, commonly called the Dead Sea.*

EXPERIMENT I.

*Read Feb. 25.
1741-2.*

UPON steeping or infusing some Scrapings of Gall in it, (after standing a long time) it turn'd of a bright purple Colour.

EXPERIMENT II.

Upon the Instillation of *Ol. Tartari per Deliq.* it immediately became troubled or muddy, and seem'd as if Goblets of Fat were fluctuating in it. This unctuous Matter, upon long standing in Repose, came

came gradually into closer Contact, and at last subsided.

EXPERIMENT III.

Upon the Instillation of *Spirit of Vitriol*, it deposited a Milk-white greasy Sediment; which, after 12 Hours Repose, occupied one-fifth Part of the Vehicle or Liquor.

EXPERIMENT IV.

Being mix'd with a Solution of *Saccharum Saturni*, it let fall a small Quantity of a greyish Powder.

EXPERIMENT V.

Being severally and separately mix'd with Solution of Sublimate, with *Sp. Sal. Armoniac.* and with Sugar of Violets; it neither fermented, deposited any Sediment, grew turbid, nor changed Colour; except only from the Sugar of Violets, which turn'd it of a dark Green.

OBSERVATIONS.

This Water is highly saturated with Salt, insomuch that any Measure of it preponderates fresh Water under equal Surfaces, in the *Ratio* of Five to Four.

It has also a wonderful Acrity, insomuch that being held in the Mouth for a short time, it constringes it in like manner as Alum does.

I cannot (from the above *Experiments*, and the Appearances which resulted from them) conclude, that this Water is impregnated with any thing more than mere Salt, which is of a very acrid, alkaline Nature; and something else, which may be of a compound Nature, partly sulphureous, and partly bitumi-

nous. But, to speak negatively, it may be 'presum'd, I think, that it neither partakes of Steel, Alum, nor Vitriol, nor yet of a pure, genuine Sulphur: And, consequently, as I take it, can afford no other, nor better Effects, to such as may bathe in it, than other Sea-water; except only, that its greater Degree of Salt, and superior Weight, may somewhat heighten the same Effects.

EXPERIMENTS (*by way of Analysis*) upon the *hot Spring Water near Tiberiades.*

EXPERIMENT I.

Oil of Tartar. per Deliq. 3fs. being mix'd with 3fs of the Water, it became troubled and muddy; and after standing 12 Hours in Repose, Three-quarters of the Whole, from the Bottom upwards, appear'd like white Wooll: But this woolly Water, being separated by Filtration, and left to dry, seem'd no other than a yellowish Oker.

EXPERIMENT II.

I mix'd 3fs *Sp. Vitriol* with 3fs of the Water, and, after 12 Hours standing still, I found a large Sediment of a white unctuous Matter.

EXPERIMENT III.

Solution of *Sublimate* 3fs being mix'd with 3fs of the Water, it became turbid and yellowish, and yielded an earthy Sediment in small Quantity; whence I conclude it contains a *Sal murale*.

E X P E R I M E N T I V.

One Ounce and half of the Water, mix'd with 3fs of a Solution of *Sacch. Saturni*, deposited a greyish Sediment of a lateritious Matter, in small Quantity.

E X P E R I M E N T V.

One Ounce and half of the Water, mix'd with 3fs *Sp. Sal. Armoniac*, turn'd turbid, of a Colour betwixt Green and Blue; and after 12 Hours Repose, yielded a woolly Sediment of four Digits deep.

E X P E R I M E N T V I.

One Ounce and half of the Water, mix'd with 3fs *Sacch. Violar.* became troubled, and of a dark-yellowish Colour.

E X P E R I M E N T V I I.

One Ounce and half mix'd with 3fs of Scrapings of Gall, became of a fine violet Colour; but when shook, was as deep as Ink.

O B S E R V A T I O N S.

This Water (as appears to my Judgment) contains a good deal of a gross fix'd Vitriol, some Alum, and a mural Salt of a limy Quality.

'Tis too salt and nauseous for internal Use; but by bathing in it, must be good for all cutaneous Distempers, and especially for the Scurvy and Leprosy: For it will powerfully deterge, scour, and clean the excretory Pores; and it may, by its Weight and *Stimulus*, restore them to their natural State, Strength, and Elasticity. It may, by the same

Means, restore the lost or impair'd Tone of the Solids in general : In Consequence of which, it may thin the Blood, help its Circulation, and promote the natural Digestions and Secretions ; and thus, finally, it may be useful in Rheumatisms, Dropsies, Jaundices, and nephritic Diseases.

Analysis of the Hammam Pharoan Water, near Corromondel, on the way to Mount Sinai.

EXPERIMENT I.

This Water being mix'd with the Scrapings of Gall, manifested no sensible Change at first ; but after long standing it grew somewhat greenish.

EXPERIMENT II.

Upon the Infillation of *Sp. Sal. Armoniac.* it became turbid ; and on standing some time in Repose, deposited a dark-greyish Powder, in small Quantity.

EXPERIMENT III.

Four Ounces of the Water, being mix'd with 3iss *Sacc. Violar.* manifested no Change, except what would necessarily result from the Tincture of Violets.

EXPERIMENT IV.

Being mix'd with a Solution of *Sacch. Saturni*, it became immediately very turbid ; but on standing some time in Repose, it deposited a large dark-brown Sediment, leaving the Vehicle troubled and whitish.

E X P E R I M E N T V.

I mix'd a Solution of *Sublimate* with it, upon which it became immediately yellow; but, after standing at Rest, it deposited a woolly unctuous Matter, in small Quantity.

E X P E R I M E N T VI.

Being mix'd with *Ol. Tartari per Deliq.* it became of a chyly Colour and Substance, or of a turbid pearly Colour.

E X P E R I M E N T VII.

Being mix'd with *Spirit of Vitriol*, it manifested no Change, either of Colour or Transparency.

O B S E R V A T I O N.

I conclude from the *Phænomena* which appear'd upon Analysis, that this Water is impregnated with a good deal of a gross earthy Sulphur, a neutral Salt, a small Quantity of Alum, but no Proportion of Vitriol.

This cannot be used inwardly, it being nauseous beyond Expression: It smells somewhat like rotten Eggs, but much worse. But, used by way of Bath, it may cleanse the Skin of all Foulnesses, purge and deterge the cutaneous Glands from all foul noxious Humours: It may reinforce the natural Heat and Vigour, (where they are decayed) and may restore the impair'd Digestions: And hence, finally, it may promote Virility in Men, and Fecundity in Women. It may likewise be useful in the Gout; as also in Epilepsies, and other Diseases of the nervous Class.

XV. *An Account of the Case of William Payne, with what appeared upon examining his Kidneys and Bladder, when his Body was opened; by Mr. George Bell, Surgeon.*

*Read Feb. 4.
1741-2. This
should have
been inserted at
p. 27. Art. X.*

William Payne, aged about 71, had been afflicted with the Stone in his Bladder, and other calculous Complaints, for several Years: He had taken Mrs. Stephens's Medicines for 15 Months. See Hartley's *View*, &c. p. 8. Case III.

He was subject also to a scrotal Rupture on the Left Side, from which however he suffered no great Inconveniency, unless upon Neglect of his Truss, which he had been directed to wear; and even then, if the Intestines came down, he used to return them with Ease.

About the Beginning of *January* last, he was attacked with a severe Fit of the Stone, attended, upon every Attempt to make Water, with a strong *Tenesmus*, that forced into the *Scrotum* a considerable Quantity of the Intestines, which exceeding his Skill to reduce, he sent for me. I found the Tumour large and unequal, but without much Tension or Inflammation; his Pulse low, with clammy Sweats; he complained of violent Pains in his Back, propagated thro' the whole Length of the *Ureters*, accompanied with Nausea and Vomitings; he felt exquisite Pain about the Neck of his Bladder and *Glans*, with an unusual Weight in *Perineo*; he had frequent Inclinations to make Water, but seldom made above a Spoonful at once, and that Drop
by

by Drop, with much Pain, and sudden Stoppings: The Urine was extremely fetid, sometimes mixed with purulent Matter, at others tinged of a Coffee Colour.

He had received, just before I saw him, a Clyster, which produced two Stools, and encouraged me to hope might facilitate the Reduction of his Rupture. I attempted it by all necessary means possible, but without Success: For altho' the largest Part receded and gave way, yet a considerable Portion remained, which I could not possibly return. I therefore concluded; as the Intestines performed their Office, and were free from Tension, Inflammation, &c. that the Parts adhered; so left him, with Directions for a Bag-Truss to support them.

January the 22d, being informed of his Death, I applied for Leave to open him, which was granted. In examining the Contents of the *Abdomen*, I found the Left Kidney quite wasted, scarce any thing remaining except the Coats, and those filled with Blood and purulent Matter; the *Ureter* very much enlarged above its natural Capacity, and full of the same.

The Right Kidney was ulcerated in several Places, and full of purulent Matter, mixed with Grit; several Hydatids appeared upon its external Surface; the *Ureter* was somewhat enlarged.

I next examined the Bladder, which was exceeding large, and contained above three Pints of clear Urine; upon opening it and introducing my Hand, I found two smooth flattish Stones, somewhat larger than common *Windsor Beans*: I discovered a third in the Neck of the Bladder, which probably had been forced there during the Paroxysm, and appeared to me to be
the

the immediate Cause of his Death : It was about the Size of a *Filbert*, and had quite corked up the Passage.

Upon dissecting the Hernial Bag, the first Part that presented was a large Piece of Fat, about half a Pound ; and immediately underneath it lay a large Portion of the *Colon*, in Length about 10 Inches ; the internal Surface of the *Peritoneum* was strongly attached to the *Colon* by several Filaments, and to the *Scrotum* by its cellular Substance.

All the other *Viscera* were in a natural State.

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ADVERTISEMENT.

A New Map of the *Virgin Islands*, taken from an actual Survey made at the Expence of the Honourable *Gilbert Flemming*, Esq; Governor of *St. Kitts*. Sold by *W. Mount* and *T. Page*, Stationers, on *Tower-hill*, *London*.

The *Gardener's best Companion* in a *Green-house* ; or, A *Table*, shewing the greatest *Heat* and *Cold* of most Countries ; with a *Catalogue* of *Exotic Plants*, under the Name of each Country which produces them. Likewise, *Thermometers*, *Hygrometers* and *Barometers*, either for common Use, or adapted to the Use of *Green-houses*, made and sold by *Tim. Sheldrake*, at the *Black-Boy* over-against *Cecil-street* in the *Strand*, *London*.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*, 1742.

The CONTENTS.

- I. *A Method of preparing Specimens of Fish, by drying their Skins, as practised by John Frid. Gronovius, M. D. at Leyden.*
- II. *A Letter from Capt. William Gordon to Capt. Samuel Mead, F. R. S. inclosing an Account of the Fire-ball seen Dec. 11. 1741.*
- III. *Extract of a Letter from the Rev. Mr. William Gostling to Peter Collinson, F. R. S. concerning the Fire-ball seen Dec. 11. last, and the Mock-Suns seen the 19th of the same Month.*
- IV. *Some Observations upon Mr. Sutton's Invention to extract the foul and stinking Air from the Well and other Parts of Ships, with Critical Remarks upon the Use of Windsails, by William Watson, F. R. S.*
- V. *Part*

THE CONTENTS.

- V. *Part of a Letter from Monsieur Claud. Joseph Geoffroy, F. R. S. to David Hartley, M. A. F. R. S. containing his Method of making Soap-lees and Hard Soap, for Medicinal Uses.*
- VI. *A Letter written to the most Reverend Father D. Cla. Fremond Calmad. publick Professor in the University of Pisa, giving an Account of the Earthquakes felt in Leghorn, from the 16th to the 27th of January 1742. With some Observations made by the most Reverend Sig. Pasqual R. Pedini, Principal of the Clergy of the most eminent College of the said City. Communicated to the ROYAL SOCIETY by James Jurin, M.D. F. R. S. &c.*

I. *A Method of preparing Specimens of Fish,
by drying their Skins, as practised by John
Frid. Gronovius, M. D. at Leyden.*

There are requisite for this Purpose,
Read at a **A** Pair of Scissars, with very fine
Meeting of the Blades, and sharp Points.
Royal Society, Small wooden Plates (of the
on March 4. Lime-tree, or wooden Trenchers).
1741-2.

A very fine Needle.

Slips of Parchment as large as the Fishes.

Minnikin Pins, or small Pins.

Take hold of the Fish with your Left Hand, so as that the Belly may be towards the Hollow of your Hand, and its Head pointed to your Breast. Then with the Needle make a Wound behind its Head, into which introduce one of the Points of your Scissars, cutting gently from thence along to the Tail. If you would preserve the Right Side, the Scissars are to be conducted on the Left Side of the Fin. This being done from the Head to the Tail, the Scissars are to be pointed deeper, and the Flesh divided quite to the Back-bone. Then turn the Fish with its Back downward, and its Belly upward, and proceed in the same manner, cutting with the Scissars through both Head and Jaws. Take away the Brain and Gills. The Fish then easily parts, the Intestines appear, which may be easily taken away. The Back bones are then to be cut asunder, the Fish is to be washed, rubbed till it is dry with a Linen Cloth, and placed upon a Board, in such a manner as that the Skin,

H

covered

covered with its Scales, may lie uppermost, and all the Fins and Tail are to be expanded with Pins. Let it then be exposed to the Sun, if in Summer, or, if in Winter, to the Fire, till the Skin grows quite dry and hard, when it must be turned, and the Flesh exposed to the Sun or Fire, till it is also dry; and then the Skin may be separated from the Flesh with very little Trouble, and, being put betwixt Papers, must be pressed flat. But as a sort of glutinous Matter, in pressing, is always forced out from betwixt the Scales and the Skin, a Piece of Parchment is to be laid under the Fish, which is easily separated from the Scales, but Paper always sticks: For this Reason it is necessary, that after an Hour or two, a fresh Piece of Parchment should be applied: And thus, in the Space of 24 Hours, the Fish is prepared.

II. *A Letter from Capt. William Gordon to Capt. Samuel Mead, F. R. S. inclosing an Account of the Fire-ball seen Dec. 11. 1741.*

S I R,

*Read March 4.
1741-2.*

AT your Desire I have sent you a Description, as exact as possibly I can remember, of the Meteor which I saw on *Friday* the 11th of *December*, coming by Water from the City to *Whitehall*.

I really at first took it for some artificial Fire-work, but was soon undeceived by the different Forms it appeared in, and the Routs it took through the Regions of the Air. I find it was seen by several People
here,

here, as well as in the Country, but little otherwise observed concerning it, but that it was like a Ball of Fire, and appeared over such and such a Place.

If my Eyes were not deceived, it put on several Forms; but as its Motions were sometimes pretty quick, I have ventured only to assure you, that the Account I have inclosed is as near the Truth as the Observations of such sudden *Phænomenons* will generally admit of.— I am with the greatest Respect,

London,
Feb. 12. 1742.

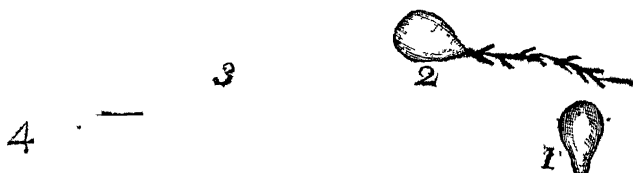
*Your most obedient,
humble Servant,*

William Gordon.

. On *Friday* the 11th of *December* 1741. about One *P. M.* coming by Water from the City to *Whitehall*, and near to *Hungerford-stairs*, there appeared to me between *Vauxhall* and *Lambeth*, a Body of Fire: It sprung upwards in its Ascent almost perpendicular to the Horizon, to the Height, as near as I could guess by my Eye, of 35 Degrees, in the Space of a few Seconds, and nearly in Form of a large Boy's Kite, projecting a long Tail towards the North-west, not unlike those of Slips of Paper set on Fire: In this State it continued so long, that I made the Waterman lay his Oars in, that I might the more easily observe whether it was the Work of Art or Nature, for I was in some Doubt. It had from its first Appearance expanded itself considerably, so that the extreme Breadth was seemingly equal to the Diameter of a Full Moon arising from a dusky Horizon. In this Form it continued ascending for the Space of

Two Minutes, gently shooting withal to the North-east, till it arose to about 45 Degrees; then suddenly quitting its Tail, which vanished, colouring the neighbouring Clouds with a Yellow on their Separation, it formed itself first into a Ball of Fire; then shooting quickly to the South-east in a Stream of Light, disappeared, making a Noise like a Clap of Thunder at some Distance, and leaving behind it a smoaky Substance in its Tract.

The Weather moderate and cloudy, Wind, as high as I can remember, West South-west. It continued in Sight upwards of Five Minutes.



III. *Extract of a Letter from the Rev. Mr. William Gostling to Peter Collinson, F. R. S. concerning the Fire-ball seen Dec. 11. last, and the Mock-Suns seen the 19th of the same Month.*

My good Friend,

Canterbury, Mar. 9. 1741-2.

*Read March
18. 1741-2.*

AS the Fire-ball appeared at Noon-day, and the Sun shining, few People saw it, and they could only guess at the Course.

Course. The best Account I have had is at Second-hand, from two Farmers who saw it together, and make its Course from North-west by North to South-east by South, and right over *Littleborn* (which is the first Village in the Road from hence to *Deal*). Their way of telling its Course was by saying, it went from *Westbere* toward *Ratling*, near which Place Lord *Cowper* was then hunting, and heard but one Explosion, which seemed to be within a few Rods of him: The other, I suppose, happened at such a Distance, as to be in one with that so near him.

I have inclosed a Sketch of an Appearance we had eight Days after the Ball, *viz.* Dec. 19. of which, I believe, the Duration was uncommon, from Sun-rise to Noon *.

I remember *Dampier* mentions *Haloës* as presaging Storms, and this Combination of them has been succeeded-by a long Season of stormy Weather. I am

Your obliged Friend and Servant,

William Gostling.

* The Representation of it was exactly the same as that at p. 47. of N^o 462. except that the Light at *c*, expressed in that, was omitted in this.

IV. *Some Observations upon Mr. Sutton's Invention to extract the foul and stinking Air from the Well and other Parts of Ships, with Critical Remarks upon the Use of Windsails, by William Watson, F. R. S.*

London, Dec. 4. 1741.

*Read April 1.
1742.*

AS nothing is more conducive to the Health of the human Body, than taking a sufficient Quantity of wholesome Air into the Lungs, so the contrary is attended with pernicious and often with destructive Consequences.

One of the great Uses of Air in Inspiration is, to cool the Blood passing through the Lungs, where Nature has provided, according to the excellent *Malpighius*, that the Blood should be distributed through a vast Number of exceedingly fine Arteries, which are applied all round the thin Vesicles of the Lungs; and by this means the Blood is exposed to the Air under a prodigious large Surface, whereby the Putrefaction is prevented, which, from the *alcalescent* Quality of that Fluid, would otherwise be speedily destructive.

Observations inform us, that contagious Distempers are more frequent in hot Climates than cold; and in closely built Cities fully inhabited, than in Towns: The former may, in some measure, proceed from the too great Heat of the Air, not fully answering the above-mentioned Purposes; and the latter from too many People breathing in the same Atmosphere, thereby rendering it unfit for Respiration.

It has been frequently tried, that if a Gallon of Air be contained in a Bladder, and by means of a Blow-pipe inspired and expired into the Lungs of a Man, without having any Communication with the external Air; in the Space of a Minute, or little more, it becomes heated, and unfit for Respiration; and without the Addition of fresh Air, the Person would speedily be suffocated. The Diving-bell is another Instance of the same kind, where a constant Supply of fresh Air must be had, to keep out the Water, and refresh the People included.

Although Air is absolutely necessary to our Existence, and Necessity constrains us inevitably to breathe therein, it may be made a Vehicle of most malignant Poisons, as the famous *Grotto del Cani* in *Italy*; poisoning Air by Charcoal, Air impregnated with the Fumes of fermenting vegetable Liquors, stagnant Air, either alone or mixed with Water, soon becomes pernicious, and very offensive; as in Wells dug for Supply of Water, and disused for some time; also in the Wells and in the Holds of Ships, where what is usually called the Bulge-water, if the Ship is tight, and the Water not pumped out often, it soon becomes so extremely poisonous, as frequently to suffocate those Seamen, who, as the Pumps are subject to be clogged with Filth, venture down to cleanse them; and also to affect Persons at a Distance with violent Head-achs, cold Sweats, and frequent Vomitings, which continue more or less, in Proportion to the Distance from the Well of the Ship when the Injury was received, and the Degree of Putrefaction in the Water and Air.

The Air, in Ships particularly, is very liable to be vitiated; not only from the Bulge-water, but from too many People breathing in the same Atmosphere; especially in Ships of War, Hospital-ships, and those used in the *Guinea Trade* for *Negroes*, where a Number of uncleanly People, being stowed too close together, heat the Air, make it replete with noxious *Effluvia*, destroy the Particles therein adapted to cool the Lungs, particularly the acid nitrous *Gas*, which is so abundant in cool Air, and manifests itself not only from the Quantity of nitrous Crystallizations, which may be collected from Caverns of the Earth, especially those open to a Northerly Aspect, but from exposing Pieces of the Flesh of Animals fresh cut, or their Blood, whereby the Colours of their Surfaces are soon changed from a dark deep Red to a more lively and florid one. Air robbed of this valuable Property, and replete with hurtful ones, not only from the People, but from the stinking Water in the Well and lower Parts of the Ship, must produce the most putrid, if not pestilential Fevers.

Although the *Æquilibrium* within Places confined is maintained by the external Air, yet unless, by Openings properly adapted, the Air is suffered to pass freely through, the external Air proves as a Stopple to the internal, and only mixes with the next in Contact; as is evident from the common Occurrence in Privies, which are scarcely offensive in clear Weather, but are much so in foul or windy, from a Diminution of the incumbent Pressure, when the Vapours that have been pent up, expand themselves to a considerable Distance.

To prevent the above-mentioned Inconveniencies, and preserve the Healths and Lives of that valuable Part of the Nation, the Seafaring People, many Schemes have been thought of; particularly the Machines of those two very worthy ingenious and industrious Members of this SOCIETY, the Rev. Dr. *Hales*, and the Rev. Dr. *Desaguliers*; the first by an Instrument which he calls *the Ship's Lungs**, and the latter by a Machine †, which is an Improvement of the *Heffian* Bellows: But as these have been laid before the SOCIETY by the Gentlemen themselves, I shall pass them over, and proceed to mention the Contrivance commonly made use of, I mean the Wind-sails. They are made of the common Sail-cloth, and are usually between 25 and 30 Foot long, according to the Size of the Ship, and are of the Form of a Cone ending obtusely: When they are made use of, they are hoisted by Ropes to about Two-thirds or more of their Height, with their Basis distended circularly by Hoops, and their Apex hanging downwards in the Hatch-ways of the Ship; above each of these, one of the common Sails is so disposed, that the greatest Part of the Air, rushing against it, is directed into the Wind-sail, and conveyed, as through a Funnel, into the upper Parts of the Body of the Ship. These must be hung up and taken down every time they are used, and the Supply by this Method is not constant. Though Custom has given a Sanction to this Device, it is subject to many Inconveniencies: 1st, Each Ship having com-

* See Dr. *Hales's* Treatise of Ventilators.

† See these *Transactions*, N^o 437.

monly three of these, (one to each Mast) the Seamen are a considerable time in getting their Apparatus ready, and hoisting them up, to make use of. *2dly*, They can only be used in mild Weather. *3dly*, Near the Equator, where fresh Air is most wanted, there sometimes happen such stark Calms, that they are useless by not having Air enough to distend them. *4thly*, The Air hereby admitted passes only into the upper and more open Parts of the Ship, so that the Well, &c. receive no Change therefrom; and it is observed, that sometimes, upon using them after some Discontinuance, they drive offensive Air into the Cabin, and more airy Parts of the Ship; like as the pouring some fresh into stinking Water makes more Water stink, though in a less Degree. *5thly*, They are improper to be used in the Night-time, when the People are sleeping between Decks. And, *Lastly*, admitting they had none of the former Inconveniencies, their Use must be destructive in Hospital ships; where, though fresh Air imperceptibly received is absolutely necessary to preserve the Crew as free as possible from the infectious Breath and Exhalations of the diseased and wounded Seamen, yet Blasts of Wind, pouring imperiously into the very Places where the Sick lie, must be attended with such Consequences as are too obvious to mention.

To remedy these Inconveniencies, to prevent Air proving foul even in the Wells and Holds of Ships, and to cause imperceptibly a large Circulation of fresh Air into every Part of the Ship at all times, Mr. *Sutton* has invented the following Scheme*, which is

* See these *Transactions*, N^o 462 p. 42.

useful not only in these Cases, but, by altering some Parts, as particular Places require, may be applied to Houses, close Parts of Prisons, Wells at Land, Prisons, Hospitals, &c.

Nothing rarefies Air so considerably as Heat, and whenever it causes a Diminution of the Density of the Air, that Part next in Contact will rush in, and be succeeded by a constant Supply, till the Air becomes of an equal Degree of Elasticity. Therefore, if a Tube be laid in the Well, Hold, or any other Part of the Ship, and the upper Part of this Tube be sufficiently heated to rarefy the impending Column of Air, the *Equilibrium* will be maintained by the putrid Air from the Bottom, which being drawn out this way, a Supply of fresh Air from the other Parts of the Ship will succeed in its Place; which Operation, being continued, will intirely change the Air in all the Parts of the Ship. This Principle, exactly conformable to the Doctrine of Pneumatics, is the Basis of Mr. *Sutton's* Machine, which being put in Execution on board the *Hulk* at *Deptford*, before the Lords of the Admiralty, Commissioners of the Navy, our very learned and ingenious President *M. Folkes*, Esq; Dr. *Mead*, &c. performed to their Satisfaction, in bringing Air from the Bread-room, Horlop and Well of the Ship at the same time, in such Quantity, that large lighted Candles being put to the End of Tubes, the Flame was immediately sucked out as fast as applied, though the End of one of the Tubes was above Twenty Yards distant from the Fire. The Method is as follows:

To boil the Provisions of the Ship's Company, they must have a Copper which is bigger or less, in proportion

portion to the Size of the Ship, and Number of the Crew : This Copper is fixed in Ships in the Manner as on Land, having under it Two Holes divided by an iron Grate. The first Hole, having an iron Door, is for the Fire ; the Ashes from the Grate drop through into the Bottom of the other ; the Smoke passes through a Chimney, and is discharged as usual. After the Fire is lighted, it is supported by the Air from the Parts next the Ash-pit ; but having, contrary to the usual Custom, adapted an iron Door, like the former, made very tight, to prevent the Ingress of Air, the Fire would soon be extinguished, if, not supplied by some other Aperture ; in order to which, one or more Holes are made through the Brick-work in the Side of the Ash-pit ; and Tubes of Lead or Copper, fitted closely in the Holes, and made fast, are laid from thence into the Well, and other Parts of the Ship ; by which means the Air next the Bottom of the Tubes rushes through them, and the foul and stinking Air succeeding is transmitted through the Fire, and passes off, without offending, by means of the Chimney ; and a Supply of fresh Air from the other Parts of the Ship continually fills the Place of the former, the Fire requiring a constant Support, which Support will be wanting, not only during the Continuance of the Fire, but while any Warmth remains in the Fire-place, Copper, or Brick-work, as was observed on board the *Hulk* at *Deptford*, where the Draught of Air through the Tube lasted above Twelve Hours after the Fire was taken away. This being considered, as the dressing the Provisions for a Number of People will take up some Hours every Day, the Warmth of the Brick-work and Flues will con-

continue a Draught of Air from one Day to the next. Mr. *Sutton* proposes thus to circulate the Air by the same and no greater Expence of Fire than is customarily used for the Necessities of the Ship. The Operation of the Machine will be equally useful in large as small Ships; for the greater the Number of People they have on board, the larger Quantity, and longer Continuance, of the Fire will be necessary to dress the Provision; and therefore there will be required a greater Quantity of Air to support that Fire. The Size and Number of the Tubes need not be specified, because as the Consumption of Air is in proportion to the Quantity of Fire, the wider the Tube, and greater the Number, the less the Velocity of the Air, and *vice versa*.

I several times observed in this Machine, when for the sake of Observation, after the Fire was well lighted, and the lowest iron Door left open, that the Flame did not ascend so high, or burn so fierce; but immediately upon shutting thereof, when the Draught of Air was only through the Tubes, the Flame soon recovered its former Vigour.

There is likewise, especially in large Ships, not only a Copper, but also a Fire-grate like those used in Kitchens: That the Heat and Smoke of this also may not be useless, an iron Tube may be fixed behind the Grate, and inserted quite through the Brick-work, and through the Deck, so that one End thereof will stand about a Foot, or little more, in the Chimney above the Brick-work, and the other will enter into the Hold, or any other Part of the Ship; so that the upper End being heated, the Draught of Air will be supplied from below, as in the other Case. This like-

likewise was tried on board the *Hulk*, with an iron Tube about Two Inches and an half in Diameter, and the lighted Candles held at the Bottom of this Tube were extinguished as fast as by any of the other.

It may be objected, that a Number of Tubes take up too much Room, especially in Merchants Ships, and are subject to be broken or injured by loading or unloading: To remedy which, it is adviseable, that only one Tube of a convenient Size be made fast unto the Side of the Ash-pit, and, as soon as it comes through the main Deck, to compress it (a circular or any other Form being equally useful) not too close; and it may be divided into as many Ramifications as may be thought necessary, (especially as the Broad-room, Store-room, &c. cannot be kept too sweet, a Branch for each of these) and these Branches be carried between the Beams which support the Deck, till they come to the Side of the Ship, and there let down likewise between the Beams into the Places intended; by which Contrivance their Operation will not in the least be obstructed, and the Tubes be secured from any Accident.

The Simplicity of this Machine, it being so little cumbersome, its Operation without any Labour to the Seamen, the small Expence to put it in Execution, and maintain it, besides the before-mentioned Considerations, are other Arguments for its general Use.

V. *Part of a Letter from Monsieur Claud. Joseph Geoffroy, F. R. S. to David Hartley, M. A. F. R. S. containing his Method of making Soap-lees and Hard Soap, for Medicinal Uses.*

S I R, *Paris, July 23. 1741. N. S.*

*Read April 1.
1742.*

— **T**O make the Lye, I take, for Instance, of the best calcined Lime, that has been the least exposed to the Air, 5 lb; of good Salt of *Kali* or Glass-wort of *Alicant*, pulverized, and passed through a fine Sieve, 10 lb. I divide the Lime and the Salt of Glass-wort (called in *England Barillia*) into two equal Parts; then I put the Lime, broken into Pieces not bigger than an Egg, into new stone Pans, and cover it with as much Salt of Glass-wort as is designed for each Pan. I pour afterwards on these several Mixtures hot Water by little and little, to give Time to the Lime to open itself, till it turns into a sort of Meal, which will happen after I have poured Three half Pints * into each Pan. I then add to it the rest of the Water that is required, stirring this Mixture with a Stick of white Wood; when there are Eighteen or Nineteen Quarts of Water in each Pan, there is enough for dissolving the Salts. In this State the Pans are left for Twelve or Fifteen Hours; after which this Lye is filtrated

* The *Paris* Pint is near a Quart *Englisb*.

through

through brown Paper, supported by a coarse Cloth, fixed to the Four Corners of the filtering Frame. When the whole Mass of the Lye and of the Lime is well drained, I put it into an iron Pot that is very clean, with Ten Quarts of Water, to the Quantity taken out of each Pan, and let it boil an Hour; then I filtrate it a second time. Afterwards it is put into another iron Pot that is very clean, and as it evaporates by Degrees, it is filled up again with the first Lye prepared, without boiling. I let it continue to evaporate till the Twenty-eight Quarts of Water, that have been used for making the Lye of the Mixture that was at first put into each of the Pans, be reduced to Two Quarts and half a Pint, or so long till a small salinous Film forms itself on the Top of the Lye. This Liquor turns almost black, because it corrodes the Iron; but this is no Inconvenience, as will appear hereafter. In this State of Concentration, if one lets a Drop of it, whilst it is hot, fall on a Piece of Glass, it will be very quickly covered with a fine and greasy Film, which makes it look as if it was congealed. At the Bottom of this Lye is found a Salt in Flakes, which, being melted in a Crucible, produces a *Lapis infernalis* of a strong caustic Power. One may know also, that the Lye has acquired the necessary Degree of Concentration, when, becoming more active, one sees, that the Edge of the Pot that has been wetted by it, turns red, whilst the lower Part of the Side all around, down to the Surface of the Liquor, takes a greenish Colour. Then the Pot must be taken from the Fire, and the Liquor left to cool so far as to be put into Glass Bottles without cracking them: The Bottles ought to be carefully corked,

corked, not only to prevent the Salts contracting a Dampness from the Air, which would lessen the Degree of forced Concentration, which has been acquired by the Evaporation, but also to preserve what is sulphureous, which would exhale, if the Liquor remained long exposed to the Air: For I suspect, that that sort of *Hepar*, formed by the Union of the caustic Salt with the Sulphur of the Ashes of the Glass-wort, ought not to be neglected. Now, the better to direct those who have a mind to work after these Processes, and to furnish them with the Degrees of Concentration this Lye is to have, in order to make with Oil a solid Soap out of it as speedily as possible, I take a glass Phial with a narrow Neck, and fill it with clear Water up to a Mark made on the Neck. That which I now make use of, being filled up with Water to that Mark, contains just Three Ounces: I afterwards empty it carefully, and, instead of clear Water, I fill it with that concentrated Lye as far as the foresaid Mark, and then I weigh it. If the Weight be increased Eight and an half or Nine Drams; that is, near Three Drams in each Ounce, this shews that the Lye is neither too much nor too little concentrated. An hydrostatical Balance, a Water-poise, and other Instruments, might also give this Degree; but in the Country they are not at hand, and I judged it best to point out only what is most easy. Soap-boilers use for this End a fresh Egg; if one half of it sinks into the Lye, they judge the latter to be of the first Strength, that is to say, that this is the Lye which they ought to employ last of all in their Manufacture; if the Egg sinks in to Two-thirds, the Lye is called the Second; and, lastly, if the Lye covers the whole

Surface of the Egg, it will be called the First, and will be that with which they begin their Operation or Boiling. But this way of trying has not all the Exactness which can be desired, because all Hens Eggs have not the same specific Gravity. Besides, as I make my Soap without Fire, I must take the Lye that is most concentrated.

Lest the Iron, which is corroded by the Lye, should enter into the Composition of the Soap, one need only to evaporate the Lyes in earthen Pans put over a *Balneum Mariæ*; but as this Evaporation is slower, it will consume much more Coals. One may even see in those Pans by different Marks, that the Liquor approaches the desired Degree of Concentration, partly by a Piece of Wood marked with Notches, partly because if there is the least ferruginous Speck in the Earth of those Pans, the Liquor will penetrate that ferruginous Place, and make a Spot there. By using earthen Pans you will get a very limpid Liquor, and which will only have a very pale Straw-colour, even after its perfect Concentration.

The Lye prepared in Iron, being kept for some time, clears up, and leaves a black Sediment, which is that Part of the Iron which it has separated by corroding the Sides of the Pot. And yet this ferruginous Lye, together with the Oil, will form a white Soap, if one has let that black Sediment precipitate. This Sediment is true Iron: I have made myself sure of it, by calcining it in a Crucible, after having moistened it with Oil.

One Ounce of concentrated Lye to the Degree above-mentioned contains Three Drachms Eighteen Grains of Salt; when I dissolve this Salt again in
distilled

distilled Rain-water, and filtrate it, I find in it Three Grains of coarse Earth, which cannot penetrate the Pores of the Filtre.

If I use it to make Soap of it, I take one Part to two Parts of the best Oil: I mix them gently in a *China* Bowl, stirring them with a *Spathula* of white Wood, till both Liquors are come to a Consistence of Butter that is churning: This Thickening is much sooner done in Winter than in Summer. I keep the Vessel in a dry Place, that the Moisture of the Air may not diminish the Strength of the Lye. The Mixture from Day to Day grows to a Body, and when it is in the Sun in Summer, and upon the Mantle of the Chimney in Winter, the Phlegm evaporating sooner, it becomes perfect Soap in Four or Five Days, provided the Lye be sufficiently concentrated. It will be well however, that during the time the two Liquors are binding together, the Mixture be stirred with the *Spathula*, that the Water may not be kept in, but evaporate the sooner. When the Soap is made, it easily comes out of the Vessel, but it has not yet lost all that Moisture it should lose; so that though one may use it in that State, yet it is better to keep it Twelve or Fifteen Days longer; at the End of which Time if I decompound it, I always find the whole Oil I have employed; that is to say, out of Eighteen Drachms of this perfect Soap, I get one Ounce and an half of Oil, and Two Drachms Twenty-three or Twenty-four Grains of Salt of Glass-wort. So after this Method a Patient may easily make his own Soap, and be sure of the Ingredients; perhaps even in the great Manufacturies, one Day or other, they may prefer this to that which is now in Use.

As to what relates to the Oil of Lime*, of which I have spoken in my Experiments, it is the *Caput Mortuum* of the *Sal Ammoniac*, after Distillation of the volatile Spirit by the means of Quick-lime; it is exposed in a flat Vessel to the Moisture of the Cellar, whence a *Deliquium* is formed, which we call Oil of Lime. It is Lime dissolved by the means of the Acid of the Sea-salt, which is contained in the *Sal Ammoniac*; other Chymists call it the fixed Liquor of *Sal Ammoniac*. Your Soap-boilers are obliged to add Sea-salt to their Soap, which I believe, for my part, comes from their making use of Pot-ash in their Lyes, which they would have no occasion to have recourse to, if they employed true Salt of Glass-wort, seeing my strong Lye of Salt of Glass-wort makes Soap immediately; besides, the Salt of Glass-wort contains Sea-salt, which I have demonstrated by making Salt of *Glauber* with pure Salt of Glass-wort and Oil of Vitriol: If instead of Salt of Glass-wort one makes use of Pot-ash with Oil of Vitriol, it will not make Salt of *Glauber*, but instead of it produce Tartar vitriolate.

In describing this sort of Soap, I had no other View, than not to deviate from the way of making *Alicant Soap*, and to know well the Proportions, in order to apply them to the making of the Soap I propose, and to fix them with regard to the Lime and the Salt of Glass-wort, which for many and various Reasons is preferable to other fixed Salts, as being that which forms the best, the most deterfive,

* Huile de Chaux.

and the mildest Soap, as it has been found by Experience in all our Manufacturies.

The Observations which I have lately laid before the Academy, prove that the Oil, which has passed through the Lyes of Lime and of Salts, is, perhaps, easier to digest than any other. I there demonstrate, that the Oil separated from the Soap by the means of Acids, as I have pointed out, is found to have acquired a Property which it had not before; for it dissolves in Spirit of Wine, and perfectly unites with it; which it could not do whilst it was crude, that is to say, before it had formed Soap, or had been boiled with metallic Limes.

VI. *A Letter written to the most Reverend Father D. Cla. Fremond Calmad, publick Professor in the University of Pisa, giving an Account of the Earthquakes felt in Leghorn, from the 16th to the 27th of January 1742. With some Observations made by the most Reverend Sig. Pasqual R. Pedini, Principal of the Clergy of the most eminent College of the said City. Communicated to the ROYAL SOCIETY by James Jurin, M. D. F. R. S. &c.*

Read April 8.
1742.

I N Obedience to your Commands, I transmit you an Account of the Earthquakes felt in *Leghorn*, from the 16th to the 27th of *January*.

January. I suppose you have observed at *Pisa*, the unsettled Weather, as we have here; continual Rains, sometimes more, and sometimes less; continual Winds, chiefly from the South and West, so that scarce any in this City remember to have seen the like. We cannot be said to want Wind here, since scarcely a Day passes without it, from some Point or other. But to observe in the Country one Wind in the Morning, another at Noon, and then a third at Night, is what surprises us. In some Days, especially after the Winter Solstice, there have been felt extraordinary warm Southern Breezes, from whence People drew an ill Omen of the Health, or other Misfortunes, which every one figured according to his own particular Fancy: However, no one thought of the present Calamities, the Reasons of which are unseen by mortal Eyes; for where shall we find those Telescopes through which our Sight may reach the subterraneous Receptacles of that Matter, which, whether burned or fomented, makes the whole Earth start, and terrify Man? I look upon the Foresight of these Accidents, as an Undertaking impossible to accomplish, and the prophetic Fixing them to a certain Time, much more so: But observe to what a pitch Ignorance or Fear carries some People; after the first Shock of the Earthquake, every body was in great Consternation within this City, not so much for what they immediately felt, as through Fear of another infinitely more violent, which a certain *Milanese* Astrologer predicted to happen (as they said) the 28th of *January*. By Misfortune he was within a Day of it, the great Earthquake being the 27th; by this means the Faith and Credit given to the Astrologer increased
so

so much, that I do not know whether he has not more Reverence and Honour shewn him than the Prophets, and holy Gospel. There is no need to prove, that this Science does not belong to Astrologers; for Effect shews it, since the Earthquake came a Day before his Prognostication. He has moreover predicted another Earthquake to happen the 6th of *March* next, upon which Numbers who are in the Country, and some at *Pisa*, will not return to *Leghorn* till that Day is past.

The 16th of *January* was a very temperate Day, with a gentle Breeze between South and West: A little after Twenty-four Hours (about Six at Night, according to our *English* way of reckoning) I observed a certain dark Cloud, which passed with a bad Smell; of this I took but little Notice, having often smelt the like; and what might occasion a greater Inobservance, was, a great Cold, which prevented my distinguishing between Smells, whether good or bad: However, I saw this Cloud, blacker and thicker than the rest, settle within a Foot and an half on the Tops of the Houses, like the Smoke that the Peasants make in an Evening, when they burn their Garden Rubbish, or such-like. On account of the Cold I had, and this black Cloud, I went into a Friend's House: Finding him with Company, after a little common Discourse, he returned, it being *Tuesday*, and I remained with a few more, At Two Hours in the Night, (Eight o'Clock *English*) we thought the Pavement gave way, and the Chamber shook: Some of us thought it proceeded from walking in another Chamber, others thought it was a Shock of an Earthquake; upon which I listened

at-

attentively, to hear if there might be any consequent Motion or Noise in the House, that I could attribute it too; but on the contrary every thing was quite still: Upon this I went to the Window, and found a small Air from the South; the dark Cloud was no longer to be seen, but a thin slight Obscurity in the Air. Scarce a Quarter of an Hour passed, but the Chamber received a more violent Motion than the former, though not to frighten us very much. I observed a Motion in the Candles on the Table from West to East. We then heard all the Bells in the City; on this my Friends and I went out of the House, and staid abroad till Four Hours. I smelt the Stink no longer, but observed the Clouds increasing and thickening on every Hand, but always with a white Hue, like the Circle which is often seen round the Moon, but of a prodigious Extent. Every body's Eyes were busied at this, looking on it as the Forerunner of something extraordinary, though nobody knew what would be the Event. Many asked my Opinion of this Novelty: I told them, I had observed the same thing very often, and that the Consequence was sometimes Rain, or Wind, and very often nothing at all: In short, I persuaded them it was nothing out of the common way, and did not portend any future Evil, as they thought. But returning home a few Minutes after Four in the Night, I was got about half way up the Steps, when another violent Shock began, far superior to the two former, which lasted about the time one might say an *Ave, Maria*; the Motion was sudden, and the shaking of the House was from East to West: The House wherein I live being South, and I standing fronting it, staggered twice to the Right Hand,

Hand, and in great Danger of falling down all the Stairs.

At Ten Hours and a half, were heard by many two other Shocks, (with a small Intermission of Time) much like the two first; however, I was not sensible of these. One waked me at Eleven and Three quarters, and another about an Hour after: These were perceived by every body, but were of no Consequence; and I being between Sleep and Wake, could not tell which way the House moved. In the Morning the 17th of *January*, after the last Shock, there fell a little small Rain, like Hail, which turned to Snow about 14 or 15 Hours, which fell in such Abundance for an Hour, that the Streets and Tops of the Houses were quite covered; and a little more after Mid-day, which continued all the Remainder of the Day. On the 18th there were no sensible Shocks of the Earthquake, but there were now-and-then visible Undulations of the Ground, though of no Consequence. The 19th in the Morning, at Sunrise, there were between the East and South certain Clouds very thick, which dispersed as they came nearer to the Solar Disk; but there always remained a particular uncommon whitish Thickness in the Air, till Sixteen Hours, when it was intirely dissipated; a small Gale rose from the South, which soon fell again, and changed to the West; the Sun was so scorching, that it racked the Head to stay in it. At Eighteen Hours and a half, I heard a rumbling Noise, which seized me with Horror, and expected an Earthquake was at hand, neither was I deceived, the House began to shake, and continued the Motion Eight to Ten Seconds: It came like a Blow, and the House

L

waved

waved from West to East. At Nineteen Hours exactly, followed another Shock, which lasted about Three Seconds; but I did not observe any Motion of the Building, being so surrounded by Numbers of People, that I could not stir, nor raise myself from kneeling, being then at Church. All the Remainder of the Day, quite till Twenty-three Hours, the Earth was in continual Motion; and exactly at Twenty-three Hours followed another Shock, like that at Eighteen Hours and a half. I felt nothing at all of this, by the Increase of the People, who poured in upon us. However, at Two Hours, Three and a half, and Three Hours Fifty Minutes, I perceived Three small Shocks; and from that Time to the 20th of *January* at Twenty-three Hours and a half, I felt nothing: At this Hour there was a small shuddering, which was not universally observed. At Five Hours Twenty-five Minutes in the Night, followed a Shock like that of Eighteen Hours and a half of the 19th Day, with this Difference only, that the House waved from South-east, and continued between Ten and Twelve Seconds; there followed a strong Undulation of the Ground (something interrupted) until Twenty Hours of the 21st of *January*; at Twenty-three Hours of the said Day, being in the great Piazza of the City, I found a small Motion of the Ground, which was observed by a few more, that I happened to be in Company with.

Some Fishermen told me, that at the same Hour on the 19th Day of *January*, that we had the terrible Shock at *Leghorn*, they being at Sea between *Meloria* and *Gergona*, saw a small Part of the Sea rage violently, and raise itself to a great Height in a white
Foam,

Foam, with a dreadful Roaring, and frightened them so far as to imagine themselves lost, though it did not directly beat upon them, but felt it on one Side only; which made them imagine some violent Mischance at Shore; and keeping their Eyes always on that troubled Part of the Sea, perceived it made towards *Leghorn*, and broke on the old Fortrefs, which for a little while was hid from them. The Captain of a Ship, who came to this Port, says, that he saw, to his great Surprize, a few Miles distant from *Capo Corso*, several Streams running with great Impetuosity different ways, and so very rough, that although he had a very fair Wind, he expected every Moment to be lost. This must have happened just before the Earthquake of the 19th of *January* above-mentioned.

From the 20th to Twenty-three Hours of the 25th of *January*, the Ground was in a continual Agitation: I suspected it might be my foolish Apprehensions: I asked every one I saw, but every body agreed there was some little Matter. To assure myself of the Truth, I put Water in a Bason, and put it on a Plain, observing it every time I thought I felt any thing, and saw it move: I continued this Observation till the 26th of *January*, and at Twenty-three Hours on the 25th Day, there was a much greater Motion than that of the 20th Day; and from that Hour until Eighteen Hours Three-quarters on the 27th Day, there was not the least Motion perceptible: Upon this I hoped the Evil was past, and comforted myself with thinking that Matter spent to which the Philosophers attribute this horrid *Phænomenon*; but found myself mistaken, for when I least expected it,

and my Mind quite otherwise engaged, I was surprised the said Hour with a most dreadful Noise, which was followed by a treble Shock of the Earthquake in the most frightful Manner, and beyond measure violent; it began by a succussive Motion, and followed by a sort of Blow with the horrible Violence; and at last came another succussive Motion, more horrible than the former: There was heard from under-ground a hollow terrible Rumbling, as if the whole Earth had broken to Pieces: It had a Motion like turning, and continued moving; the Houses waved Thirty or Thirty-two Seconds, from East to West. I looked upon myself quite lost, and expected nothing less than the immediate Ruin of the House, especially when I beheld part of the Door-case falling, and the Partition-walls cracked; the Mortar fell all about like Rain, the Furniture and Cloaths hung to the Walls fell all down; in this I was confined, without being able to seek Safety out of the House, but stood fixed, and nailed up, (as it were) by the surrounding Crowd of frightened Wretches that flocked in upon me: At last, however, I got out, and could hardly believe my Eyes, when I found the Houses all standing, having figured things much worse than I found them; yet every thing has suffered very much, there not being a single Edifice but what is damaged; although a great part of the Hurt within the Houses proceeds chiefly from the Roguery of the Builders, who either when first built, or in repairing, used bad Materials; those which are well built have suffered scarce any thing: Some must inevitably be rebuilt, chiefly those which remain leaning to one Side; which proceeds chiefly from the Load occasioned by
their

their being raised so high. What has most surprised me is, the Number of Cracks in the Walls of this Collegiate Church, which were built without sparing any Cost, to make them a complete Piece of Workmanship, and are of an extraordinary Thickness, as one may observe in some of the Openings in the Building and Vaulting, which was esteemed superior to any in this Town: From hence you may conceive a just Idea of the extreme Violence of the roaring Earthquake. As for myself, I look upon it as a particular Providence, that the whole City did not go to Wreck; and had not the Houses been in general very good, they must have come to the Ground. The Ruins consist in, *viz.* the Roof of the Church of *St. John Baptist*, the Convent of *Augustine* Friars, the Roof of a Palace called *Rosciano*, belonging to the Family of *Borghesi* of *Sienna*. Besides these there are few others of Consequence, and but Three People killed. There is an immense Quantity of Iron Chains used, to keep the Walls of the Houses together.

Upon account of the Inconveniences attending this Earthquake, an infinite Number of People went out of the Town; the Houses and Shops were abandoned instantly, to seek Refuge in the great Piazza: So great was the Consternation, that no one knew what he was about. It was an Object of the greatest Compassion, to see the Astonishment and general Confusion that prevailed; every body looked pale as Death, without knowing what he did or said. There was another small Shock at Nineteen a Second, at Nineteen One-quarter, and a Third at Twenty Hours: After this last, I staid till Twenty-one Hours of the 30th of *January*; and then went away to breathe a little

little of pure and more quiet Air that you enjoy, and observed no further considerable Motion of the Earth; there remained, however, a continual Undulation, sometimes more, sometimes less; but must own, that from Twenty-two Hours of the 27th of *January* to Thirteen Hours One-half of the next Day, I could not perceive any thing, because I retired, and went to lie on board a Ship.

We may observe here, that some Earthquakes happen in cloudy, some in serene, some in still, and others in quite stormy Weather. The 16th of *January* at Night, was Snow and Clouds, as above-noted, with a very small South Wind from Midnight to Break of Day; the Fogginess turned into Clouds, which afterwards became Sleet and Snow. On the 19th in the Morning, was a bright Sun, but a gentle Breeze; about Twenty-three Hours it was cloudy, which at last covered all the Sky, continuing cloudy all that Day and the next Night, when at Five Hours Twenty-five Minutes followed the above-mentioned Earthquake; and in the Morning about Thirteen Hours, there fell a small Sleet and a Westerly Wind. Before the Earthquake on the 19th, the Waters swelled, and then fell again; soon after they swelled half a Yard higher than they ever were used to do. I was told by many, that the same Night and the following, there was a strong Smell of Sulphur in the Streets; but my Cold prevented its being perceived by me. This Smell was likewise found in the Water of some Wells. The Sea was seen in sundry Situations, now high, and then presently very low again; sometimes strongly agitated, and at others on a sudden calm. On *Sunday* the 20th of *January*, a small
Sleet

Sleet fell all Day, and the Air was changeable till the 27th in the Morning, being by turns, serene, cloudy, foggy, windy, and damp, with South and Westerly Winds. The 27th in the Morning, was a pleasant fine Sky, and a bright Sun, but excessive hot : About Sixteen or Seventeen Hours, a brisk Westerly Wind arose, and with this full Wind we sustained, at Eighteen Hours and a half, the violent Shock of the Earthquake ; the Waters were observed to rise as high or something higher than the 19th. At Night, between Twenty-four Hours and One Hour in the Night, it became cloudy in the West, with a strong Wind ; from Midnight to Day-break, fell a small Sleet, which continued (with some Interruption now-and-then) till the 28th Day ; between whiles the Sun shone. The 28th at Night, and the 29th Day, it rained violently, accompanied by strong blustering Winds from the West. The 29th it was all Day cloudy, with the same violent Wind and Rain. The 30th the Sun began to appear, but the Clouds were not all dispersed. At Twenty-one Hours this Day I left *Leighorn*, and have not been able to make any further Observations.

It is said here, that the Sea roared with such Violence and Smartness, that its Noise was like the firing of large Cannon. I have not seen any body who was then at Sea, but a Friend of mine informed me, that a Fisherman (a *Frenchman* by Nation) being then in his Boat, found it of a sudden raised up a prodigious Height, and then it fell down so low, that he thought it had touched the Bottom of the Sea, and concluded himself lost : During this uncommon Motion he affirms to have heard one of these Noises resembling the

the firing a Cannon, and afterwards felt no Storm. I give it neither for true or false, but as a Relation of others. It is assured me by many, that on the Ninth Hour of the 10th Day, there was a small Shock of an Earthquake. These are all the Observations I have been able to make myself, and gather from other creditable Persons, having avoided the additional Stories that are commonly raised on such Occasions.

What has much attributed to the Preservation of this City, is the fatherly Care and Solitude of our Royal Sovereign, who, by the Means of his Royal Council of Regency, neither has or will ever fail giving us Instances of his Royal Munificence to this afflicted City; having ordered, that such Wood, Iron, &c. should be furnished as may be necessary for Repairs, with certain fixed Prices; having further ordered one half of the Duty to be taken off of Flesh. He has also, at his own Expence, sent a most able Engineer, and two Master-builders, to estimate and supervise the said Repairs: Neither has his Bounty failed to the more Indigent, who not having wherewithal to repair the Damages they have suffered by this Earthquake, he has ordered Money to be distributed for repairing them, that he may again see this his dear City vested in its former Beauty. The Assistance and Watchfulness of the Civil and Military Power was likewise very great, even during the time of the Earthquake; for by their means there were no Disorders practised, not even in the midst of the Hurry and Confusion, as it very commonly happens upon such Occasions.

You may always command me, and I am intirely at your Service, but hope never to obey you in such

a disagreeable Relation as the present. With me well, as I do you. *Adieu.*

Leghorn, Feb. 13. 1742.

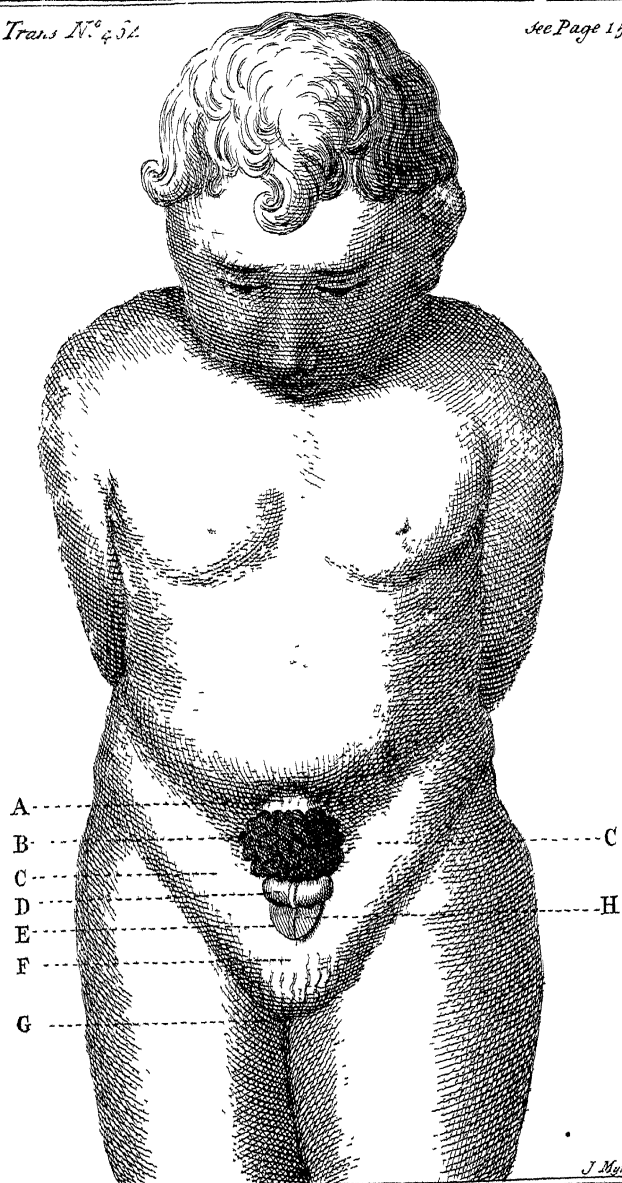
P O S T S C R I P T.

I cannot omit to acquaint you with some Observations communicated to me after the writing of this Letter, by Sig. *Ferdinando Tidi*, a Gentleman of incontestable Credit, who being at his Seat in the Country, called *Popogna*, pretty high up a Hill, between the Mountain *Montenero* and the Valley *Benedetto*, on the 20th of *January*, about Two Hours and an half in the Night, he observed a large Circuit of Air in the West, (quite from the Island of *Corfica* to *Capo Mele*) thickened with Clouds, but open, and all the Remainder of the Sky covered with heavy dark Clouds; he saw the Air light, and extremely shining, so that one might easily read a Book; and, according to his Description, must have been a very bright *Aurora Borealis*. Besides this, he observed that when we had the Wind from South or East, the Sea was in great Agitation, and ran towards *Leghorn*, but suddenly retired. I will likewise tell you, that Sig. *Guiseppe Vincenti*, Captain of the first *Lazaretto*, and present *Cansaloniere* of the City, a Person not easily to be imposed upon, having one Night, at about Three Hours, opened his Window, saw a Cloud in the West, which was exceeding dark, except in the Middle, where a strong Light (like the Influence of the Sun just before its Rise) discovered itself, and dispersed Beams of reddish Fire all over the Circumference of the Cloud, which was very extensive. He made a Friend of his observe the same thing, but

neither one or the other remember what Night it happened; and being uncertain of the particular Night, is the Reason I omitted it in the Account: But since it happens, that I am treating of these *Phænomenons*, which may have some Relation to the Earthquake, and proceed from the same first Cause, I imagine it must have been the same Night, though perhaps not. However it may have been, I have related to you a true Exposition of all things as they really were; and there is now a way opened for Philosophical Observations and Inquiries. As for me, I should be of Opinion, that it is a Collection of those Vapours and Exhalations proceeding from the Fermentation or Lighting of those Particles of Matter, which occasioned the Earthquake; but should be obliged to you for your Sentiments on it, having a great Regard to your Opinion. I remain as above.

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N. B. N^o 456. of these *Transactions* is ready to be published in a few Days, and the Gap between, that and N^o 462. will be filled up with all possible Speed, the Papers being ready for the Press.



PHILOSOPHICAL TRANSACTIONS.

For the Months of *May, June, and July*, 1742.

The CONTENTS.

- I. J. Castillioneus *D^{no}*. *De Montagny, V. C. Philosophiæ Professori in Academia Lauzannensi, REGIÆ SOCIETATIS Londinensis Membro dignissimo, S^{ti} Evangelii Ministro, &c. &c. S. P. D.* *Pag.* 91.
- II. *Two Histories of Internal Cancers, and of what appeared upon Dissection, by William Burton, M. D.* 99.
- III. *Enarratio Observationum circa Rorem deciduum, factarum Medioburgi in Zeelandis, a Leonardo Stocke, M. D. super aperta Plantie plumbea Turris Astronomica Joh. Munkii, Architecti publici, tempore nocturno, inter 25 & 26 Julii 1741. N. S. cum figuris Floccorum Nivalium Jan. 1742. ibidem observatorum.* 112.
- IV. *A Letter from Martin Triewald, F. R. S. Captain of Mechanics, and Military Architect to the King of Sweden, to C. Mortimer, M. D. Sec. R. S. concerning the Vegetation of Melon-trees Forty-two Years old.* 115.
- V. *Samuelis Christiani Hollmanni, Leg. Met. & Theol. Natural. in Regia Georgia Augusta, P. P. O.*
ad

The CONTENTS.

- ad Cromwellum Mortimerum, M. D. Regalis Societ. Secr. Epistola de Differentiis Altitudinum Barometrorum.* 116.
- VI. *A Letter from John Huxham, M. D. to Dr. Mortimer, Sec. R. S. concerning Polypi taken out of the Hearts of several Sailors just arrived at Plymouth from the West-Indies.* 123.
- VII. *An Extract of a Topographical Account of Bridgnorth in the County of Salop, communicated to the ROYAL SOCIETY by the Rev. Mr. Stackhouse, Minister of St. Mary Magdalen in that Town; containing an Account of the Situation, Soil, Air, Births and Burials of that Place, and of some Tumuli Sepulchrales near it.* 127.
- VIII. *Part of a Letter from the Right Hon^{ble} Robert James Lord Petre, F. R. S. to Martin Folkes, Esq; Pres. R. S. concerning some extraordinary Effects of Lightning.* 136.
- IX. *An Account of a Meteor seen at Peckham, Dec. 11. 1741. by T. Milner, M. D. Sec No 462-3.* 138.
- X. *Some Conjectures concerning Electricity, and the Rise of Vapours, by J. T. Desaguliers, LL. D. F. R. S.* 140.
- XI. *An Account of Margaret Cutting, a young Woman, now living at Wickham Market in Suffolk, who speaks readily and intelligibly, though she has lost her Tongue, by Henry Baker, F. R. S.* 143.
- XII. *A remarkable Conformation, or Lusus Naturæ, in a Child; by C. Warwick, Surgeon, in Truro, Cornwall.* 152.

I. J. Castillioneus D^{no} De Montagny V. C.
Philosophiæ Professori in Academia Lau-
zannensi, REGIÆ SOCIETATIS Londinenfis
Membro dignissimo, S^{ti} Evangelii Ministro,
 &c. &c. S. P. D.

*Read at a
 Meeting of the
 Royal Society,
 on May 6.
 1742.*

NEMO ignorat Newtonianam formulam, qua *Polynomium* quodcumque, ope binomii assumpti, ad quamvis potestatem extollitur; sed nemo, quod sciam, eam demonstravit. Hoc ego facere conatus meditatuunculas meas tibi æquissimo & optimo Judici mitto. *Tu, corrige, sodes, hoc dic, hocque, parum claris lucem dare coge, arguito ambigue dictum, mutanda notato.*

Continet hoc Problema tria prorsus diversa, quæ cum diversimode gignantur, & cum optima demonstratio e rei natura, vel genesi ducatur, diversa quoque probatione sunt confirmanda: Siquidem index est aut *integer*, aut *fractus*, uterque demum vel *positivus*, vel *negativus*.

1. Index sit *integer*, & *positivus*, tunc binomium ad potestatem cujus index est *m* elevare, nihil aliud est, quam toties binomium datum scribere, quoties unitas est in *m*, & omnia hæc binomia invicem ducere.

2. Si index est *fractus*, & *positivus*, binomium elevare ad potestatem $\frac{r}{n}$ est, datum binomium elevare ad potestatem *r*, & hac potestate data, quærere quantitatem, quæ data ad potestatem *n* æquat ipsam dati binomii potestatem *r*.

3. Cum vero Index est *negativus*, five is *integer*, five *fractus*, ut binomium elevetur, facienda sunt, quæ supra No. 1. vel 2, & deinde per inventam potestatem unitas est dividenda.

Sumo Binomium $p + q$, ut indicet mihi quodvis Polynomium.

Inter p^m , & q^m tot sunt medii Geometrici, in ratione $p \cdot q$ quot unitates in $m - 1$.

Hos terminos inventurus noto, quod p^m est ad q^m in ratione composita ipsius p^m . 1, & 1. q^m , ut & p ad q habet rationem compositam ex p . 1, & ex 1. q ; sed si fiant duæ series potestatum, in quarum altera indices ipsius p decrescant eadem proportionem arithmetica, cujus differentia est 1, qua crescunt in secunda serie indices ipsius q , habebitur series continue proportionalium in ratione p . 1, & 1. q .

Sic $p.1 :: p^m \cdot p^{m-1} \cdot p^{m-2} \cdot p^{m-3} \cdot p^{m-4} \dots p^{m-m} = p^0 = 1$

1. $q :: 1 \cdot q \cdot q^2 \cdot q^3 \cdot q^4 \dots q^m$.

Ergo terminis respondentibus invicem ductis

$p \cdot q :: p^m \cdot p^{m-1} q \cdot p^{m-2} q^2 \cdot p^{m-3} q^3 \cdot p^{m-4} q^4 \dots q^m$

Nunc dico $\overline{p + q}^m$ componi ex terminis supra inventis, ut facile ex genesi probatur.

Ergo omnes termini, qui sunt in $\overline{p + q}^m$ ordine dispositi sunt in proportionem continuam.

Et quidem duo quivis sese immediate sequentes sunt, ut primus binomialis radices terminus ad secundum.

Quod patet ex genesi, nam p aliquoties ductum est ad q toties ductum in p , ut $p \cdot q$.

Igitur omnium numerus est $m + 1$; sed & in serie arithmetica decrescante $m. m - 1. m - 2. \dots 0$ termini sunt numero $m + 1$, aut crescente $0. 1. 2. 3.$

$\dots m$;

. m ; ergo termini componentes $\overline{p+q}^m$ debent habere indices hos, aut esse $p^m, p^{m-1}q, q^m$.

Atqui ex legibus multiplicationis numerus terminorum debet esse $2^m > m+1$, ergo in hoc facto aliqui termini repetiti debent inveniri.

Vulgaria facta (ea, nempe, quorum multiplicans & multiplicandum constat quantitatibus diversis) omnes continent diversos terminos, quia omnes formantur diversis factoribus. In potestatibus ergo dispiendum quinam termini diversi essent, nisi factores semper essent iidem, & quot ex diversis restitutione litterarum æquales fiant; sic enim reperiemus quoties quisque in potestate repeti debeat.

Jam patet, quod si factores semper essent diversi, diversi quoque essent omnes termini in producto.

Quod cum primus in producto non fiat nisi ex primis multiplicantium, & ultimus illius ex horum ultimis, semper hæc facta erunt diversa, quamvis binomia facientia sint eadem, quia primus binomii terminus differt a secundo.

Quod ex cæteris aliqui possunt fieri æquales, quia constanter ex primis facientium ductis in secundos, & diversimode junctis.

Igitur quærendum est, quot diversis modis jungi possint quantitates, quarum numerus datus est.

In casu nostro index rerum est m , res diversæ duæ, quarum una repetitur vicibus s , altera t , ita ut $s+t = m$; ergo numerus permutationum erit

$$\frac{m.m-1.m-2.m-3 \dots 1}{s.s-1.s-2 \dots 1.t.t-1.t-2.t-3 \dots 1}$$

Sic sit $t=1, s=m-1$, terminus erit $p^{m-1}q$, & ejus

coefficientis $\frac{m.m-1.m-2.m-3 \dots 1}{m-1.m-2.m-3 \dots 1} = m.$

Sit $t=3, s=m-3$; habebitur coëfficiens ipſius

$$p^{m-3}q^3, = \frac{m.m-1.m-2.m-3.m-4}{1.2.3.m-3.m-4.m-5}.$$

$\frac{2-1.m-2}{1.2.3}$, & ſic de cæteris.

1.2.3.

Si quis forte dubitet, an ſuperior demonſtratio evincat omnes terminos neceſſario formari tot modis, quibus poſſunt, & contendat eam tantum oſtendere id accidere poſſe, hoc reſponſi ferat.

Certe $\overline{p+q}^m = \overline{p+q} \times \overline{p+q}^{m-1}$; ſed inter hujus terminos ſunt $p^{m-n-1}q^n$, & $p^{m-n}q^{n-1}$, quæ neceſſario ducentur in p & q , & $p^{m-n-1}q^n \times p = p^{m-n}q^n = p^{m-n}q^{n-1} \times q$, ergo $p^{m-n}q^n$ omnibus modis poſſibilibus factum erit in $\overline{p+q}^m$, ſi $p^{m-n-1}q^n$ & $p^{m-n}q^{n-1}$ ſint genita quot modis poſſunt in $\overline{p+q}^{m-1}$; quod neceſſario erit, ſi $p^{m-n-2}q^n$, & $p^{m-n}q^{n-2}$ ſint in inferiori poteſtate $\overline{p+q}^{m-2}$, & ſic ſemper uſque ad quadratum in quo pp , pq , & qq habentur, efficta tot quot poſſunt modis (4. II. *Euclid.*) ergo & in ſuperioribus.

Hoc ratiocinium monet, ut idem etiam ſic oſtendam, ratione paulo diverſa.

Jam primi coëfficientem eſſe unitatem demonſtravimus.

Secundus terminus $p^{m-1}q$ conficitur ex $p^{m-2}q \times p$, & $p^{m-1} \times q$, id eſt, ex primo radicis in ſecundum ipſius $\overline{p+q}^{m-1}$, & ex ſecundo radicis in primum $\overline{p+q}^{m-1}$, ergo in $\overline{p+q}^m$ adeſt $p^{m-1}q$ ſemel, plus toties, quoties ſecundus eſt in $\overline{p+q}^{m-1}$, qui ibi eſt ſemel, plus toties, quoties ſecundus in $\overline{p+q}^{m-2}$, qui ruruſus ibi eſt ſemel.
plus

plus toties, quoties secundus est in $\overline{p+q}^{m-2}$, & sic semper donec deveniatur ad $\overline{p+q}^{m-m}$, ubi semel est secundus; ergo quærenda est summa tot unitatum, quot sunt in m , quæ est m .

Item tertius $p^{m-2}qq$ conficitur ex $p^{m-3}qq \times p$, tertio $\overline{p+q}^{m-1}$ in primum radicis, & ex $p^{m-2}q \times q$ secundo ipsius $\overline{p+q}^{m-1}$ in secundum radicis; ergo $\overline{p+q}^m$ continebit $p^{m-2}qq$ quoties secundus continetur in $\overline{p+q}^{m-1}$, id est, $m-1$ vices, plus toties quoties ibidem astat tertius, id est, quoties secundus est in $\overline{p+q}^{m-2}$ ($m-2$) plus quoties ibi est tertius, qui rursus est quoties secundus est in $\overline{p+q}^{m-3}$ ($m-3$) plus quoties ibi est tertius, atque ita porro donec perveniamus ad $\overline{p+q}^2$ ubi semel est tertius, aut ad $p+q$, ubi tertius nullus est; nam semper quærenda est summa progressionis arithmeticæ $m-1, m-2, m-3, \dots, 1$, aut $m-1, m-2, \dots, 0$, in illa numerus terminorum est $m-1$, in hac m , ut patet; quare hæc summa $= \overline{m-1+1} \times \frac{m-1}{2} = m \times \frac{m-1}{2} = \overline{m-1+0} \times \frac{m}{2}$.

Eodem pacto coefficientes reliquorum terminorum probabuntur efficere seriem in qua secundæ differentiæ sunt in progressionem arithmetica, &c.

Unde semper, ubi m est integer, & positivus, formula crit

$$p^m + m p^{m-1} q + \frac{m \cdot m-1}{2} p^{m-2} q^2 + \frac{m \cdot m-1 \cdot m-2}{2 \cdot 3} p^{m-3} q^3 + \frac{m \cdot m-1 \cdot m-2 \cdot m-3}{2 \cdot 3 \cdot 4} p^{m-4} q^4 + \dots + \frac{m \cdot m-1 \cdot m-2 \cdot m-3 \cdot m-4}{2 \cdot 3 \cdot 4 \cdot 5} p^{m-5} q^5, \text{ \&c.}$$

Si fiat $p+q = p \times 1 + \frac{q}{p}$, hinc orietur ipsissima New-
toni formula; nam $\overline{p+q}^m = p^m \times 1 + \frac{q}{p} \Big| =$

$$p^m \times 1 + \frac{m}{1} \times \frac{p}{q} + \frac{m \cdot m-1}{1 \cdot 2} \times \frac{p^2}{q^2} + \frac{m \cdot m-1 \cdot m-2}{1 \cdot 2 \cdot 3} \times \frac{p^3}{q^3}, \&c. =$$

(si $A, B, C, D, \&c.$ ponantur æquare primum, secundum, tertium, quartum, $\&c.$ cum suis quemque coefficientibus)

$$p^m \times 1 + mA \frac{q}{p} + \frac{m-1}{2} B \frac{q}{p} + \frac{m-2}{3} C \frac{q}{p} \\ + \frac{m-3}{4} D \frac{q}{p} + \frac{m-4}{5} E \frac{q}{p} + \frac{m-5}{6} F \frac{q}{p}, \&c.$$

Quæramus nunc formulam elevandi ejusdem binomii ad potestatem $\frac{r}{n}$, ubi r & n sunt numeri integri, & ambo vel positivi, vel negativi.

Jam $p \cdot q :: \overline{p^n}^{\frac{r}{n}} \cdot x = \frac{\overline{p^n}^{\frac{r}{n}} q}{n} = \overline{p^n}^{\frac{r}{n}-1} q$, quare termini erunt

$$\overline{p^n}^{\frac{r}{n}-1} q \cdot \overline{p^n}^{-2} q q \cdot \overline{p^n}^{-3} q^3, \&c.$$

Coefficientes inveniendi sint A, B, C, D, E , ita ut

$$\text{totum } \overline{p+q}^{\frac{r}{n}} \text{ radix} = A \overline{p^n}^{\frac{r}{n}} + B \overline{p^n}^{\frac{r}{n}-1} q + C \overline{p^n}^{\frac{r}{n}-2} q q + \\ D \overline{p^n}^{\frac{r}{n}-3} q^3 + E \overline{p^n}^{\frac{r}{n}-4} q^4, \&c. \text{ ergo } \overline{p+q}^{\frac{r}{n}} (p^r + r p^{r-1} q + \\ \frac{r \cdot r-1}{2} p^{r-2} q q + \frac{r \cdot r-1 \cdot r-2}{2 \cdot 3} p^{r-3} q^3, \&c.) = A \overline{p^n}^{\frac{r}{n}} + \\ B \overline{p^n}^{\frac{r}{n}-1} q + C \overline{p^n}^{\frac{r}{n}-2} q q, \&c. \Big| = A^n p^r + n A^{n-1} B p^{r-1} q + \\ n A^{n-1} C p^{r-2} q q + n A^{n-1} D p^{r-3} q^3 + n A^{n-1} E p^{r-4} q^4 \&c. \\ + n \cdot n -$$

$$\begin{aligned}
 & + \frac{n \cdot n - 1}{2} A^{n-2} B^2 p^{r-2} q q + n \cdot n - 1 A^{n-2} B C p^{r-3} q^3 + \\
 & n \cdot n - 1 A^{n-2} B D p^{r-4} q^4 \& c. + \frac{n \cdot n - 1 \cdot n - 2}{2 \cdot 3} A^{n-3} B^3 p^{r-3} q^3 \\
 & + \frac{n \cdot n - 1 \cdot n - 2}{2} A^{n-3} B^2 C p^{r-4} q^4 \& c. + \\
 & \frac{n \cdot n - 1 \cdot n - 2 \cdot n - 3}{2 \cdot 3 \cdot 4} A^{n-4} B^4 p^{r-4} q^4. \text{ Atque ideo collatis} \\
 & \text{terminis } 1 = A^n = A^{n-1} = A^{n-2} \& c. nB = r, \& B = \frac{r}{n}, \\
 & nC + \frac{n \cdot n - 2}{2} \times \frac{r r}{n n} = \frac{r \cdot r - 1}{2}, \& C = \frac{r \cdot r - n}{2 \cdot n n}, nD + \\
 & n \cdot n - 1 \times \frac{r}{n} \times \frac{r \cdot r - n}{2 n n} + \frac{n \cdot n - 1 \cdot n - 2}{2 \cdot 3} \times \frac{r^3}{n^3} = \frac{r \cdot r - 1 \cdot r - 2}{2 \cdot 3}, \& \\
 & D = \frac{r \cdot r - n \cdot r - 2 n}{2 \cdot 3 \cdot n^3} \& c.
 \end{aligned}$$

Si ergo faciamus $\frac{r}{n} = m$, & primum terminum A ,
 $\& c.$ revivet prior formula, & $\overline{p+q}^r = \overline{p+q}^m = p^r \times$
 $1 + m A \frac{q}{p} + \frac{m-1}{2} B \frac{p}{q} + \frac{m-2}{3} C \frac{q}{p} \& c.$

Extollendum fit binomium $p+q$ ad negativam po-
 testatem, seu perfectam, seu imperfectam—s.

$$\begin{aligned}
 \text{Jam } \overline{p+q}^{-s} &= \frac{1}{\overline{p+q}^s} = \frac{1}{p^s + s p^{s-1} q + \frac{s \cdot s - 1}{2} p^{s-2} q q \& c.} \\
 &= (\text{per divisionem}) \frac{1}{p} - \frac{s p^{s-1} q}{p^{2s}} - \frac{s \cdot s - 1}{2} \times \frac{p^{s-2} q q}{p^{2s}}
 \end{aligned}$$

$$\frac{s.s-1.s-2}{2.3} \times \frac{p^{1-1}q^3}{p^{2s}} - \frac{s.s-1.s-2.s-3}{2.3.4} \times \frac{p^{1-4}q^4}{p^{2s}}$$

$$p^{-s} - sp^{-s-1}q - \frac{s.s-1}{2} p^{-s-2}qq.$$

Ex hac formula facile, superiorum vestigiis insilendo, eruitur solemnis & generalissima $p^m \times 1 +$

$$mA \frac{q}{p} + \frac{m-1}{2} B \frac{q}{p} \&c.$$

Non injucundum puto, quod in hac formula, si $m=-2$, coefficientes erunt numeri naturales, si $m=-3$, trigonales, pyramidales, si $m=-4$ &c.

Cæterum constat hanc formulam semper dare seriem infinitam; siquidem (si m exponit numerum positivum) ultimus terminus esse deberet q^{-m} ; sed $p.q : p^{-m}.p^{-m-1} :: p^{-m-1}q.p^{-m-2}qq$, &c. ergo ratio ipsius $p^{-m}.q^{-m}$ componi deberet ex aliquibus rationibus

$p.q$, quod fieri nequit, quia $p^{-m}.q^{-m} :: \frac{1}{p^m} \cdot \frac{1}{q^m} :: q^m.p^m$ in ratione composita ex reciprocis ipsius $p.q$.

Quod & aliter demonstratur, indices ipsius p faciunt progressionem arithmeticam, cujus termini $-m$, $-m-1$, $-m-2$, &c. negativi quidem sunt, sed crescunt, aut ab 3 recedunt; atqui ultimus terminus debet esse $q^{-m} = p^0 q^{-m}$, ergo nunquam ad illud devenietur.

Viviaci,
postidie Id. Septemb.
C10 IDCCXXXI.

II. *Two Histories of Internal Cancers, and of what appeared upon Dissection, by William Burton, M. D.*

I.

Read May 13.
1742.

Windsor, May 22. 1734

B *Artholomew Collins*, a Labourer in the King's Works at *Windsor*, of low Stature, pale Complexion, slender and active, aged about 36 Years, temperate in his manner of Living, had, for some Years, been afflicted at different times with wandering arthritic, colic, and nephritic Pains, none of which were periodical or constant. During this Term, when in best Health, he was usually costive, and his Urine, as soon as made, deposited a *calculous* Sediment.

In *March* 1733. he received a violent Blow by a ponderous and obtuse Instrument on his Loins, together with the Spine of the *Os Innominatum*, towards the Left Side, which disabled him for that Day; on the next, the Pain abating, he continued so well for Six Months after, as not even to recollect this Accident, till about a Month before his Death, although he was often asked by the Physician, Whether that Part had ever suffered a Contusion.

In *January* following, he complained of an excruciating Pain, extending from the aforesaid Spine to the spurious Ribs on the Left Side, which sometimes attacked also the Intestines; whence he became continually restless, especially in the Night, and, tossing the Bedcloaths off, frequently lay naked.

O

He

He could not now lie upon his Back or Left, *viz.* the affected Side, but lay always on his Right Side, leaning on his Right Elbow. In *April* 1734. his Left Knee, from a Contraction of the Muscles elevating it, was always drawn up towards the *Abdomen*, insomuch that he could not stand upright. His Left *Testicle*, formerly less than the Right, was now become scirrhus, and increased to double the Magnitude of this, and the Left Spermatic Vessels felt like a knotty Chord. A sort of Hætic Fever attended him, the Exacerbation of which, as well as of his Pains, was generally about Noon, and Six in the Evening.—He had no Sleep of Nights without a Purgative.—Though his Appetite failed him, he had no Propensity to vomit, nor complained of Thirst. His Respiration and Urine were not amiss.—His Pains were always exasperated by the Use of heating Medicines; and whenever the Pain seized the Intestines, terebinthinate Clysters increased them, whereas emollient and refrigerating ones mitigated them; by the Use of which he had daily one or two Stools.—The Fæces were of a middling Consistence, lightly tinged with Bile.—The Blood frequently taken away by *V. S.* in small Quantities, had always a thick, tough, fizy Buff-like pleuritic Coat; and at first, from each several Bleeding, he found Relief.

January 4. 1733-4. The Apothecary first administered to him, for the Colic, *Elect. Lenitiv. & Pulv. Diasen. Ol. Junip.* and emollient Clysters. Three Days after, Pains seizing his Left Side, and the *Sphincter Vesicæ*, they were removed by repeated Bleedings, and *Decoct. Hord. & Lap. Prunel.* and *Syr. Althææ*. On the 12th, he complained of a
Heat

Heat about the *Regio Pabis*, with Costiveness; but by the Use of *Sal. Mirab. Glaub. Lap. Prunel. Manna*, emollient Decoctions with *Sp. Nitr. d. & Elect. Lenitiv. pro re nata*, continued tolerably easy till the 24th, when the erratic Pains returning, and not yielding to the aforesaid Apozems, on the 26th the Physician first consulted gave him *Sperm. Cet. Sal. C. C. & pulv. e Chel.* and *Sp. C. C.* but these not availing, the *Lap. Prunel.* was sometimes interposed, and a *Calomel* Bolus, taken at Night, was worked off by a Sena Potion next Day: This Method, and afterwards Powders of *Lap. Prunel.* and *Cinnab. Antim.* taken in Honey, mitigated those Pains. But from *February 3.* to *April 4.* sometimes nephritic Pains, intermitting Fever, pleuritic Pains, and Strangury, inordinately afflicted him, notwithstanding the Use, according to the said Indications, of carminative, terebinthinate and saponaceous Clysters, Purges, *Sal. Absinth.* Draughts, repeated Bleedings, Vesicatories, the Powders and Electuary above-mentioned, Opiates, *Cort. Peruv.* in an Electuary, and infused in Wine, *Lac Sulph.* with Asses Milk, *Cinnab. Antim. Millep. Gum Guaiac.* and *Pulv. e Chel.* with *Vinum Milleped.* and *Sp. Nitr. d.* On *March 20th*, *Calomel. Gr. v.* were given for Four Nights successively, and afterwards purged off with a Sena Potion, and then continued again till the 28th, when he took another Potion.—Paregorics were used now-and-then at Night, and *Ung. Opo-deldoc* was applied to the *Testicle*.

April 4th, I first attended him in Consultation, when he complained of an intolerable Pain, upon any Pressure about the Region of the Left Kidney;

whereupon a maturing Cataplasm was applied in the Day-time, and a Plaister at Night. Emulsions, Whey, and such-like, were the chief Internals he used till *April* 8th, when crude Mercury was recommended to him, of which he took an Ounce Night and Morning, which gave so much Relief as to encourage the Continuance of that Medicine only to the 17th, when the Pains returning, he was bled once in Two or Three Days, to Four or Five Ounces, and treated with the subacid, cooling Regimen, and Paregorics, till *April* 29th, when he first mentioned a scirrhus Tumour, as big as a Hen's Egg, situate on the Left Mastoid Muscle of the Neck. Upon comparing this with the *Testicle*, (neither of which Tumours were in the least diminished after Applications for that Purpose) it was conjectured, that either the Pancreas or Mesenteric Glands were cancerated. An Emetic of *Oxym. Scillit.* farinaceous Decoctions with Nitre, crude *Sal Armoniac*, *Oxym. Simp.* and *Diacod.* were of little or no Effect.—Afterwards continuing the Use of Electuaries of *Cons. Ros. r. Elect. Lenitiv. Balf. Locatel. Æthiop. Min. Sperm. Cet. Ol. Amygd. d.* and the same with *Mercur. Alcalizat.* he became more and more emaciated, till *May* 21st, the Day of his Death.—

It was remarkable, that every new Medicine, except it was very heating, afforded some Relief for Two or Three Days.

May 22d, On removing the Integuments of the *Abdomen*, the *Musculi Recti* appeared livid.—The *Omentum* was destitute of Fat.—The *Intestine* contiguous to the Left *Os Innominatum* was tinged with Green.—Nothing besides appeared morbid in the
Viscera

Viscera in Situ at first View.—The Situation of the *Pylorus* seemed lower than usual. The Colour or Texture of the Liver were not remarkably preternatural. The Spleen was of the largest Size, and adhered in its hinder Part so strongly to the *Peritoneum*, that it could not be separated without Laceration.—Whereupon there remained in the Place of Adhesion a thick, callous, and almost horny Membrane, as big as an Half-crown.—The *Pancreas* was very small, and seemed composed of small *Scirrhi*.—The Left Kidney was twice as big as the Right, or as its own natural Magnitude: Its Substance about the *Pelvis* was corroded by a semipurulent cancerous *Sanies*, that was in part collected between the Surface of the Kidney and its containing Bag. The internal Structure of it was not much amiss: But the *Fomes Morbi*, the most singular and surprising *Phænomenon* in this Subject, was a Number of large conglobate, steatomatous, cancerated Glands, reaching from the *Receptaculum Chyli* to the lowest *Vertebræ* of the Loins, so connected together as to represent a *Pancreas* affixed to the *Vertebræ* of the Loins, and upper anterior Part of the Left *Psoas* Muscle: It was Four times as large as his *Pancreas*, and as big as the Right or sound Kidney.—The *Aorta descendens* pervaded the Middle of this preternatural Substance Lengthways.—From this Mass, as a Fountain, flowed that cancerous *Sanies*, which had made its Way to the Left Kidney, and also corroded the superior carneous Part of the Left *Psoas major*, and *Iliacus internus*, so that one might easily rend their gangrened Flesh like rotten Linen.—Some of this green *Ichor* collected near the Os
In-

Innominatum had laid the Spine of it quite bare. The Left spermatic Vessels were knotty, tumefied, and livid—The mesenteric Glands were scirrhus. The descending Trunk of the *Aorta* was smaller than usual; and, dividing it, we extracted a small *Polypus*. —The Examination of the other Cavities was not permitted. —

II.

January 11. 1735-6.

Thomas Trinder, a Taylor, living at *Windsor*, in his 29th Year, was of a pale Complexion, with red Hair, of a middle Stature, and thin Habit, addicted to smoaking from Morning to Night, and now-and-then to hard drinking. Eight Years before his Death, he was thrown in Wrestling, so as to pitch the Small of his Back upon the Corner of a Chair, by which at first he was much hurt in that Part; but upon the Abatement of his Pain, he became from that time subject to Fits of the Colic, in which he said his Bowels seemed to be drawn to his Back-bone, and usually received Ease by binding his Waist as tight as he could.—He had also frequent recourse to Geneva and such Liquors for Relief, but seldom found any, till a Swelling, as big as a Hen's Egg, appeared like a Rupture in his Right Groin. These Fits were not of above 24 Hours Duration, but the inguinal Tumor lasted Two or Three Days. He was often afflicted with Stitches under his Left Breast, which were removed by Bleeding.

But in the Middle of *November* 1735. his Colic became so violent, that he could not lie in his Bed, nor sleep without Opiates.—*November* 21. I found him

him in the Use of some carminative Pills sent by an Apothecary. He had frequent Reachings to vomit, and was very costive. His Pains seemed confined to the intestinal Region, and were most acute in the Evening, continuing to harass him till Five or Six in the Morning. His Pulse beat seldom under an Hundred in a Minute, at Night generally above. He was not very hot, nor thirsty. His Urine at this Time was rather defective in Quantity, than amiss in respect of Colour or Separation.—His Tongue was foul towards the Root, but not very white.—Upon his taking *Ipecacuan.* ʒss. and after its Operation a *Sal. Absinth.* Draught, with *Syr.* and *Tinct. Rhabarb.* ā ʒij, and the plentiful Use of *Infus. Sem. Lini*, Barley-water, Broths, and Clysters of Whey, Oil, and Honey, his Reachings ceased, and the Pains descended from the *Epigastrium* to the *Hypogastrium*. On November 24th, a Potion of *Tinct. Rhabarb. cum Vino* ʒiij. *Elix. pp. Helm.* ʒj. *Sal. Absinth.* ʒss. and Two purgative Clysters, one of which had *Terebinth* ʒss *cum Vitel. Ov.* not procuring a Stool, he took a Bolus of *Calomel* ʒss & *Camphor.* gr. xij. *b. f.* and the next Day his Pains continued, though he had several Dejections from the Cathartic. Bleeding was omitted hitherto, because he had ʒx. of Blood taken away a Week before this Paroxysm; but now, upon losing so much Blood more, he found immediate Relief. His Blood was very fizy.

After this, his Disorder resembled a Nephritic one, his chief Complaint being of Pain about the Region of the Kidneys, and along the Descent of the Ureters. From November 26th, upon the Use of an oily *Linctus* and *Manna, Sal. Absinth.* Draughts
and

and Nitre, Clysters as well as Drinks of Whey and Honey, with Paregorics, and repeated Bleeding, the Pain, removing from the Loins, fixed itself at the *Os Pubis*, and in the Thighs, *November 29.* but by the Use of Emulsions with Gum Arabic and Nitre, the Pain about the *Pubis* abated, and mostly afflicted the Left Thigh and Hip. From this Time he generally sat up in his Bed, leaning forward to the Left, and for the most part cross-legged, finding himself easiest in this Posture. He could not lie any time on his Right Side.—The Quantity, Colour, and Sediment of his Urine, were much the same now as when he was in Health.—It was made without the Pain, which, soon after the nephritic Symptoms commenced, he complained of at the Root of the *Penis*. And now his Disorder resembled the *Lumbago* and *Sciatica*, affecting the Left Side mostly: Whereupon, *December 2d*, he was put in the Use of a diuretic and aperient Electuary, with *Terebinthinate* Clysters.—To this Time he had very few Stools without Clysters, and those generally very small and fetid. *December 5th*, the Fever and Pain increasing, a cooling aperient Apozem, with a paregoric Draught *pro re nata*, were continued till *December 9th*, when examining the Thigh where the Pain now afflicted him most, I found some small *Scirrhi* in the Groin, which were sensible enough to the Touch, though not to the Sight; and from that Time, apprehending him of a scrophulous Habit, prescribed as follows: *R Myrrh. Milleped. Suc. Glycyrrhiz. Terebinth. Venet. Pil. Matth. a ʒj. Bals. Gilead. ʒss Diacod. q. s. f. Pilule N° xx. Capiat ij tertiis horis cum Sero Lactis*, which giving Relief, were
re-

repeated, only exchanging *Pil. Matth. gr.* viij. for *Ol. Anis. gtt* v. and continued to *December* 12th, when his Pains returned violently, and he coughed up clotred Matter, not unlike the *Parenchyma* of the Lungs abraded, with a little Blood. His Breath became fetid, his Respiration troubled, and he complained of Thirst. He used oily paregoric Draughts to the 15th, about which Time he was seized with so violent a pleuritic Pain, in the Middle of the Night, that it was thought he must have expired, had not about 3x of Blood been taken away immediately, upon which the Pain soon removed from the Side, and attacked him there but for a few Minutes afterwards. The Blood continued as fizy as ever. In the Evenings, when his Pains were most vehement, he had been sometimes delirious.

December 18th, he first told me of a Tumor he had discovered near the Navel, since I saw him on the 15th. It appeared not as he sat, but when laid on his Back, there was a Protuberance bigger than a Turkey's Egg, Four Fingers Breadth on the Left Side of the Navel, extending Two above it, and Four below it. By its Situation, Resistance to Pressure, and the Unevenness that from under the Skin was communicated to the Touch, its disappearing when he was in an erect Posture, and its not being diminished by discutient Fomentations, it was judged to be a scirrhus Tumor, which had long existed there unobserved by the Patient, till it increased too much to be longer undiscovered.—The *Emplast. de Ran. & cum Mercur.* was applied outwardly; and concluding there was an internal Cancer, I was encouraged, from the preceding Case, to order him *Hydrargyr.*

3j every Morning.—Whereupon there was such a Remission of his Pains, that during almost a Fortnight, he got more Rest without Opiates than before with them; infomuch that being greatly revived, and regaining some Appetite, he got down Stairs Two or Three times. Thinking the Plaister increased his Pain, Indigo blue Linen was applied in its room. The Mercury came away by Stool, and he had now one almost every Day, and sometimes twice a Day, without a Clyster.—His chief Drink was Milk and warm Water.—His grand Complaint now was of a most troublesome *Cardialgia*, especially when he lay down, which was somewhat mitigated by Powders of *Cret. Britan. cum pauxillo Sal. Absinth.* From the first Use of the Mercury he seemed on the mending hand, till after about 12 Days, when omitting it for a few Days, he relapsed into his former or a worse Condition; and though he was somewhat easier on the Repetition of it, the good Effects lasted not long.—He drooped daily from the 4th of *January*, and on the 13th died, emaciated and almost *exanguis*.

Upon Dissection, nothing preternatural appeared in the Integuments, abdominal Muscles, or *Peritoneum* immediately under them.—But under all these, where the Protuberance had been observed, and immediately under the *Omentum*, (which was destitute of Fat, and its lower Part was mortified) there came in View an anomalous Substance *in Situ*, seemingly as big as a very large Potatoe; which, when the circumambient *Viscera* were removed, was found to be a scirrhus, fungous, cancerated Excrecence, rooted, as it were, to the Left Side of the
Ver-

Vertebræ, quite from the *Diaphragm* down to the *Pelvis*, of a monstrous Bulk, occupying near one half of the *Abdomen*, lying like a Tortoise with its Head towards the *Pelvis*, and its Back to the *Umbilicus*. It was in the upper Part covered by and firmly cohered with the *Colon*, which in the whole Contiguity was black and mortified. It was strongly attached to the *Peritoneum* on the Left Side of the *Lumbar Vertebrae*, having displaced the Left Kidney, and brought it forwards to the Left Side of the Navel, so that it came in View as soon as the *Omentum* was removed. It likewise removed the *Aorta descendens*, the Left Emulgent, and Meseraic Vessels, quite out of their natural Situation; all of which were found pervading the Centre, nearly, of this Excrescence, and smaller than natural.—It adhered to the Kidney strongly where the emulgent Vessels enter it, and it had detrudd most of the small Guts into the *Pelvis*.—Nothing was preternatural in the Stomach or Spleen, excepting that the latter, as well as the Left Kidney, seemed paler than usual, and this Kidney also more flaccid: The Gall-bladder was shrunk to the Size of a Nutmeg, and empty.—The Liver had a preternatural Lobule, as big as a Hazel-nut, adhering to it by a small Pedicle. But otherwise all these *Viscera*, as well as the Right Kidney, Bladder, &c. discovered nothing morbid.—

This cancerated Excrescence could not be eradicated without Laceration, and upon the Removal of it, Two or Three large Trunks of Nerves appeared naked, passing over the *Iliacus internus* to the Thigh, which had been compressed by this Tumor. The

Weight of this Excrescence was $\text{℥iv} + \text{ʒxiv}$; and allowing for what remained upon Laceration, and the Effusion upon cutting into it, it doubtless exceeded ℥v . Upon Bisection, it appeared to the Depth of half an Inch from its Surface black and gangrened, and, below that, it was all spongy, with Cavities as large as those of an Honeycomb; and from it had issued a cancerous Sanies, draining to the *Pelvis*.

Upon opening the *Thorax*, the Right Lobe of the Lungs was full of scirrhus cancerated Tubercles, from whence a Sanies had flowed betwixt it and the *Pleura*: The Left Lobe was much smaller than the Right, firmly attached to the *Pleura* and *Mediastinum*, and inseparable without Dilaceration.—It had some Tubercles also. The Heart appeared sound, but a large *Polypus* was taken out of its Right Ventricle, at the Orifice of the *Arteria Pulmonalis*.—

Another Case occurred to me cotemporary with the first of these, and so like to both of them in the antecedent Cause and Symptoms, that, could I have obtained Leave to inspect the Corpse, I am persuaded some such immediate Cause would have discovered itself.—Crude Mercury was the only Medicine in this Case also, that palliated for about Ten Days successively.

But to what Purpose so particular a Relation of Cases confessedly incurable? It is replied, That in unsuccessful Cases an accurate Diagnostic tends not a little to the Reputation of the Physician, and his Science, although thereupon he pronounces the Case incurable by all the known Methods of regular Practitioners—Especially as this Case is very likely

to be mistaken at first for an Arthritic Disorder, or a Nephritic one, either from Ulcer, *Calculus*, or both, or for an *Iliac Passion*.

The Diagnosticks therefore of a Cancer within the *Abdomen*, deduced from the preceding Histories, seem to be as follows:

A naturally slender Habit of Body, accompanied with some scrophulous or scirrhus Tumor, together with a pale Complexion, and costive Disposition: If such an one, at an Age above Twenty, has received a violent Contusion on the Loins, and, neglecting all Remedies, is some time afterwards attacked with excessive Pains, afflicting now the *Colon*, then the urinary Passages, Spine of the *Os Innominatum*, and *Pubes*, at various times, always increased by all Internals or Externals, by which the Heat of the Body is increased, especially *Terebinthinate* Clysters; but mitigated by some Singularity of Posture, in which the Patient constantly abides; if these be attended with an Hæctic Fever, without the usual Degree of Heat in the Skin, of Whiteness or Dryness of Tongue, or Complaint of Thirst, and also without Cough, high-coloured Urine, or vitiated Respiration; if accompanied likewise with an Affection of the spermatic Vessels, of the Thighs, and frequent pleuritic Pains; the Blood always abounding with tough Size; if Opiates soon lose their Effect, and only (as all other new Remedies not heating) seem to give Relief for Two or Three Days; if Cathartics take Place, and by frequent Repetition do not produce a colliquative *Diarrhœa*, and the most palliative Remedies are nitrous Salts and Mercurial; may it not be concluded with much Probability, that such a Case is owing to some such Cause? May it not be pronounced an internal Cancer?

III. *Enarratio Observationum circa Rorem deciduum, factarum Medioburgi in Zeelandis, a Leonardo Stocke, M. D. super aperta Planitie plumbea Turris Astronomicæ Joh. Munkii, Architecti publici; tempore nocturno, inter 25 & 26 Julii 1741. N. S. cum figuris Floccorum Nivalium Jan. 1742. ibidem observatorum.*

Read May 20.
1742.

DIEI 25 Julii meridie, altitudo Barometri fuit 29 poll. 2 $\frac{3}{4}$ lin. Rhénoland. Thermometri autem Fahrenheitiani 70 gradum: Cœlo sereno, Ventoque leniter flante ex occidente æstivo: At ipso observationum tempore, ab hora decima ad primam nocturnam, Barometri altitudo erat 29 poll. 2 lin. Thermometri circiter 60 gradum, flante vix ullo Vento, Cœloque itidem sereno.

In Vitrum varii generis, multum roris decedit, ita ut rotum maderet.

In Aurichalcum politum pauxillum, nec nisi tenuis Vapor.

— Aurichalcum rude & asperum, paulo plus.

— Ferrum Stanño obductum, (Belgicæ *Blz*) parum.

— ————— cœrulei coloris, multum.

— ————— asperum, admodum multum.

— ————— læve, pæne nihil.

— ————— rubiginosum, nihil.

— Argentum vivum purum, nihil.

In

In Stannum læve, nihil.

—Plumbum asperum, multum.

— — — — — lævigatum, parum.

—Argentum candidum, nihil.

— — — — — politum, nihil.

— inauratum, item nihil.

—Vas Myrrhinum cœrulei coloris, (Belg. *Blaauw Porselein*) multum.

—Scandulam lapideam, multum.

—Corbulam ex canna Indica subtiliter plexam, modicum.

— Lignum Querceum læve: albi coloris, perquam multum.

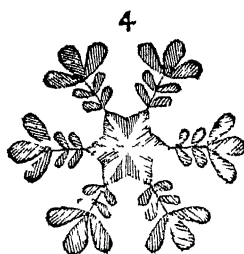
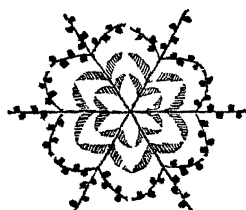
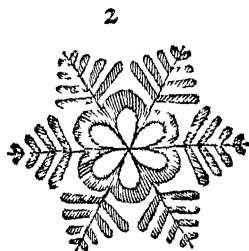
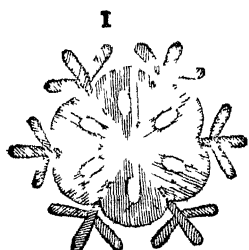
— — — — — nigri coloris, multo minus.

— — — — — Abiegnum (Belg. *Greenen hout*) lævigatum, erat tantummodo uvidum.

— — — — — albi coloris, paululum roris habebat.

—Omnis generis papyrus & humescebat.

Illis corporibus, quæ multum roris admittebant, paulo editius, distantia duarum triumve pollicum, supra locum jam roratum collatis, locus iste in plumbea turris planitie ficcabatur, ipsaque corpora tam inferne quam superne madefcebant; attamen stannum & argentum in eodem situ locata, sicca permanebant, licet ipse locus antea roratus exficcaretur.



REGIÆ SOCIETATI Lond. *Figuræ supra-*
positæ, exhibent formas Nivalium Floc-
corum, quos Medioburgi in Zeelandis ob-
servavit Leonardus Stocke, M. D.

Postridie Calend. Jan. 1742. N. S. Summo mane
depluerunt Flocci, (partim) conformati
instar Fig. 1. quorum Diametri ad ex-
trema puncta pertingentes erant $\frac{3}{4}$ lin.

10 Jan. die, ante meridiem, Fig. 2. Diametri $\frac{3}{4}$ lin.
in quibus media sexangularis rosa, sicut in
Fig. 1. ovales figuræ vacuæ erant.

20 Jan. Circa meridiem, Fig. 3. Diametri 1 lin.
& Fig. 4. Diametri $1\frac{1}{4}$ lin hi posteriores,
resplendebant lapidis specularis instar.

IV. *A Letter from Martin Triewald, F. R. S. Captain of Mechanics, and Military Architect to the King of Sweden, to C. Mortimer, M. D. Sec. R. S. concerning the Vegetation of Melon-Seeds Forty-two Years old.*

S I R,

Read May 20.
1742.

HAVING at present an Opportunity of writing, I must acquaint you with an Experiment I made this Year, relating to Vegetation, which was as follows: Secretary *Hærens*, of this Place, having a large Collection of Natural Curiosities, amongst which he has likewise collected a great Number of foreign Seeds, and finding he had *Melon-seeds* that were laid up in a Paper in the Year 1700. I was curious to try if they had retained their vegetative Quality, and accordingly the 21st of *February* last, I planted myself Twenty-four of them in a separate Hot-bed, of which I had Twenty-one good Plants, which, after they were planted in a new-made Hot-bed, shewed Flowers before they began to branch themselves, and their Branches were very narrow, yet produced early and plenty of good *Melons*. This Experiment shews not only how long *Melon-seeds* retain their vegetative Quality, but likewise that good *Melon-seeds* cannot well be too old. I know it is no new thing to make use of old *Melon-seeds* rather than new, but I never heard of any body trying so old as these. So if you think this Experiment worth taking No-

Q

tice

tice of in the *Transactions*, shall be obliged to you,
remaining, with infinite Respect,

Honoured Sir,

Your most obedient,

and most humble Servant,

Stockholm,
Nov. 16. 1741.

M. Triewald,
F. R. S.

V. Samuelis Christiani Höllmanni, *Leg. Met.
& Theol. Natural. in Regia Georgia Au-
gusta, P. P. O. ad Cromwellum Morti-
merum, M. D. Regalis Societ. Secr. Epistola
de Differentiis Altitudinum Barometrorum.*

Read May 20.
1742.

FACILE te veniam daturum spero,
vir doctissime, quod ad humanif-
imas tuas, Id. Aug. A. 1735. Lond. datas, recteque
mihi suo tempore redditas, nihil adhuc responderim.
Defuit occasio, justa etiam defuit scribendi ratio, quam
nunc casus mihi obtulit.

Eram nuper mense Julio hujus anni in eo, ut cum
Clar. *Hallero* nostro in finitimæ Hercyniæ nostræ
montes iter aliquod susciperem; ille quidem rei botani-
cæ, ego physicarum potissimum observationum, causa.
Barometrum ergo aliquod inter alia parabam, quo in
via uti commodè possem ad instituenda experimenta.
Scalam ascensus descensusque mercurii, ipsi applican-
dam, in pedis Rhenani digitos, & lineas duodecimales,
more solito, a 20^{mo} ad 32^{dum} digitum solícite divi-
seram.

feram. Cum applicare ergo illam barometro vellem, cumque in finem ejusdem, recens modo facti, altitudinem cum aliorum jamdudum paratorum, quorum *sex* numero aderant, altitudinibusque contenderem; ecce contigit mihi, quod non expectaram. Nullius enim horum omnium altitudinem altitudini alterius exacte respondere, quin discrimen potius a 2. ad 12 usque *lineas* Rhenani pedis inter easdem intercedere deprehendi: etsi nullum illorum vitio ullo laborare, fatis noveram; recens vero factum, illud quod optimum, maximeque sensibile, hætenus esse repereram, *duabus* modo *lineis* superabat. Exitinere ergo reversus die 27. Julii anni currentis in discrimina hæc observata inquirere curatius cœpi; & quoniam eodem modo eadem iterum reperiebam, *cito* nova construxi barometra, tubis rectis, sed diversas aperturas habentibus, constantia, atque inter hæc iterum discrimina ab 1. ad 4. *lineas* Rhenolandicas deprehendi; quæ maximam vero altitudinem habebant, illud quod antea optimum meum barometrum esse dixi, *sex* integris *lineis* superabant. Die 12. Aug. eadem experimenta cum his *quindecim* barometris repetii, atque eadem propemodum discrimina iterum observavi. *Alia* ergo *decem* barometra nova paravi, rectis & simplicibus tubis omnia quidem iterum constantia, ex parte tamen capsulis vitreis recurvatis, pro recipiendo ex tubis descendente mercurio factis, instructa, ex parte vero iisdem carentia. Hic vero observabam decem horum barometrorum altitudines modo a $\frac{1}{2}$ ad $1\frac{1}{2}$ *lineas* a se discrepare, & quod summum erat, optimi mei barometri altitudinem *quatuor* modo *lineis* superare.

Non opus est, ut sigillatim indicem, qua circumspeditione & cura barometra hæc omnia parata a me fuerint.

fuerint. Sufficit omnia eodem modo fuisse facta, datamque solícite esse operam, ne quicquam aëris vel in tubis extremitate resideret, vel inter mercurii particulas delitesceret, aut vitri tandem lateribus adhæreret. Octodecim vero postrema, recens facta, barometra ex eodem quoque, maximeque repurgato, mercurio facta sunt. Inter tubulos autem vitreos discrimen aliquod intercessit, quod, cum aliis nonnullis circumstantiis, silentio mihi non est prætereundum. Illud enim barometrum, quod optimum meum esse jam aliquoties dixi, atque in quo mercurius minimam semper altitudinem obtinet, tubo constat, ex vitro viridi conflato, capsulæque separatæ vitreæ, ex eodem vitro factæ, & mercurio aliquantum repletæ, insistit. Indicabo illud postea Class. I. sub No. i.; illud vero, quo in Hercyniæ montibus usus sum, sub No. ii. Ille vero tubus, in quo maxima, & integro sæpe digito Rhenano major, quam in No. i. deprehenditur mercurii altitudo, albissimo vitro constat, quod vero ubinam paratum sit, ignoro. Insertus est capsulæ ligneæ, & phosphorum habet insignem, hoc barometrum; ceterum vero omnium fere tardissime & pigerrime altitudines suas mutat. Nam non eodem modo, eademque semper promptitudine, mercurium in omnibus barometris ascendere & descendere notum jam esse existimo. Mihi saltem id ante hos septem annos jam compertum est. Indicabo hoc barometrum postea sub No. vii. Quod sub No. iii. vero postea adducam, diagonale est cum simplici curvatura, & capsula vitrea inferne adhærente, sed recurvata; sub No. iv. Bernoullianum, cujus tubus ad cylindrum, superne agglutinatum, circiter se habet ut 1. ad 8. & sub No. v. Hugenianum postea indicabo: de quo &
dia-

diagonalibus non adeo mirandum esse existimo, cum reliquis illa non conspirent. Causæ enim, cur conspirare nequeant, in aprico fere sunt positæ. N°. vi. iterum diagonale est, sed cum duplici curvatura, quarum alteram illa pars tubi excipit, cui scala applicata est cum parte tubi perpendiculari 25. circiter graduum angulum intercipiens; in cujus duplici adeo angulo ob majorem ibi tubi angustiam mercurius necessario mire in ascensu descensuque debet retardari.

Quæ ad *alteram* classẽ barometra retuli, illa omnia vitreis diversi generis tubulis constant, sed quorum massa vitrea vitio illo ex parte aliqua laborat, ut ad lampadis flammam fusa superficiem, squamulis quasi minimis coopertam, accipiat, pelluciditatisque suæ partem aliquam eo loco perdat. Atque hi ipsi tubi sunt, in quibus omnium maximam altitudinem mercurius in rectis & simplicibus barometris accepit, si solum phosphorascens excipiam.

Ad *tertiam* tandem classẽ illa barometra retuli, quæ etiam ex tubis albissimis, sed quæ nullam ab igne mutationem patiuntur, constant, quique & in eadem officina vitriaria eodem tempore parati fuerunt. Barometra, ex his constructa, omnia rectilinea, & simplicia, fuerunt. Atque in his non majorem altitudinis differentiam, quam a $\frac{1}{2}$ ad $1 \frac{1}{2}$ lineas, deprehendi, & quæ maxima in his altitudo minimam in reliquis altitudinem non amplius 4. *lineis* superavit, uti antea jam indicavi.

Num in tubulorum ergo vitreorum diversitate forsã diversitatis hujus causa quærenda erit? Num unius forsã superficies superficie alterius magis aut minus scabra & inæqualis est, mercuriique adeo ascensui frictione sua magis aut minus resistit? Aut num
alia

alia forsan adhuc rei hujus causa fuerit? Sed hoc tuo, vir doctissime, atque illustris imprimis SOCIETATIS REGIÆ, judicio totum permitto. Patiaris modo quaeso, ut brevem observationum mearum indicem adhuc subnectam.

CLASSIS I.

Barometrorum quorum altitudines diversæ die 27. Jul. & 12. Aug. observatae sunt.

	tubi apertura.	mercurii altit.	
Nº. I.	— $\frac{1}{3}$ lin.	27''	11''' ped. rhen.
II.	— $\frac{1}{2}$	28	I.
III.	— $\frac{1}{2}$	27	II.
IV.	—	28	4.
V.	—	28	5.
VI.	— $\frac{1}{3}$	28	7 $\frac{1}{2}$.
VII.	— $\frac{1}{2}$	28	9.

CLASSIS II.

Barometrorum octo, die 27. Jul. recens factorum.

	tubi apert.	mercur. altit.	
Nº. I.	— 2 lin.	28''	1 $\frac{1}{4}$ '''
II.	— 2	28	4.
III.	— 1 $\frac{2}{3}$	28	4.
IV.	— 1 $\frac{1}{2}$	28	2 $\frac{1}{2}$.
V.	— 1 $\frac{2}{3}$	28	2.
VI.	— 1 $\frac{1}{4}$	28	2.
VII.	— 1	28	2.
VIII.	— $\frac{2}{3}$	28	4.

CLASSIS III.

*Decem barometrorum, die 12. Aug. recens constructo-
rum, quorum quinque priora sine capsulis annexis
fuerunt, quinque postrema vero capsulas recurvas
vitreas, mercurio ex tubo descendenti recipiendo
inservientes, annexas inferne habuerunt.*

	<i>tubi apert.</i>	<i>mercur. altit.</i>
No. I.	— $\frac{1}{3}$ lin.	— 27'' II $\frac{1}{2}$ '''
II.	— $\frac{1}{6}$	— 27 II $\frac{1}{2}$.
III.	— I $\frac{3}{4}$	— 27 II $\frac{1}{2}$.
IV.	— 2 $\frac{1}{4}$	— 28 O.
V.	— I $\frac{1}{2}$	— 27 II $\frac{1}{2}$.
VI.	— I $\frac{2}{3}$	— 28 O.
VII.	— $\frac{1}{2}$	— 28 O.
VIII.	— I	— 28 I.
IX.	— I $\frac{1}{2}$	— 27 II $\frac{1}{2}$.
X.	— $\frac{1}{4}$	— 27 II $\frac{1}{2}$.

Quum eodem ergo tempore, eodemque in loco, aliorum barometrorum aliæ sint altitudines; annon eadem sollicitudine, ac circa thermometra adhuc factum est, de *barometris harmonicis* cogitandum prius erit, quam ex annuis barometrorum observationibus mediæ illorum in diversis locis altitudines, atque ex his, præter alia, eorundem supra marium superficiem elevationes, colligi inde satis tuto possint? Secundum Barometrum, quod in Classe I. N°. 1. posui, quo per septem annos jam hic usus sum, media altitudo mercurii hic loci erit = 27'' 10''' ped. Rhenan. secundum illud vero, quo in Hercyniæ montibus

tibus usus sum, $28'' 0'''$. ad quæ reliqua facile possunt reduci.

Ut tandem vero finiam, illud unicum mihi hic adjicere adhuc liceat, quod in Hercyniæ nostræ montibus metalliferis, summoque eorundem vertice, famosissimo sc. illo *Bruëterorum monte*, Germanice *Der Brocken*, sive, *Der Blockesberg* (Blocksberg), barometro mihi observatum est. Die nempe 9. Jul. hujus anni eo profectus sum, quum barometrum modo indicatum, hic Gottingæ subsisteret ad $28'' 3'''$. die sequenti 10. vero, in summo vertice Bruëterorum montis confedit ad $25'' 2'''$. quum interea temporis altitudinem suam ad duas lineas hic solum mutasset. Differentia ergo altitudinum inter hunc locum, & Bruëteri verticem, qui altissimus ex longe lateque circumjacentibus montibus est, est $= 2'' 11'''$. quibus secundum Cl. *Scheuchzeri* calculum, in *Transact. Philos.* N^o. 405. propositum, 2550 pedes Parisini, aut numero rotundo, 2500 circiter responderent: quæ altitudo, etsi hujus montis accolis satis ingens videtur, cum Helvetiæ tamen, Galliæ, aliarumque regionum montibus contendere nullo modo potest.

Sed ut ne te diutius morer, vir doctissime, ut res tuas porro ex voto agas, mihi favere pergas, oro, contendoque. Ita vale. Dab. Gottingæ A. 1741. die 1. Sept.

VI. *A Letter from John Huxham, M. D. to Dr. Mortimer, Sec. R. S. concerning Polypi taken out of the Hearts of several Sailors just arrived at Plymouth from the West-Indies.*

S I R,

Read May 20.
1742.

I Have taken the Liberty to send you the following Account, which, I think, contains something very remarkable.—If you think it worthy the Notice of the ROYAL SOCIETY, I beg you will lay it before them.

During the exceeding dry, cold Weather in *February* and *March* last, several of the Men brought Home in the *Deptford* and *Dunkirk* Men of War from the *West-Indies*, were seized with short, importunate, asthmatic Coughs, without any Expectoration—violent and almost continual Palpitation of the Heart, with a perpetual intermitting, trembling, fluttering Pulse, and a constant Anxiety, Pain and Sinking of the Heart, as they expressed it.—They breathed with excessive Difficulty, and could scarce lie down in Bed without Suffocation.—Their Heads, as it were, sunk between their Shoulders, and they had very dead, heavy Countenances.—Some had Pains of the Side, though very little apparent Fever.

Upwards of Twenty Persons were in a very short Time carried off towards the End of *March* in this Manner, notwithstanding the most proper and diligent Care, by Bleeding, Vomiting, Blistering, Attenuants, Diluents, &c.

R

Upon

Upon this, Mr. *Wyatt*, first Surgeon of the Hospital, who is not only a very ingenious, but careful Practitioner, ordered Two of the Dead to be opened forthwith; they were about Forty Years old.—He found monstrous *Polypi* in both their Hearts, and directly had the Hearts carried to his own House, and soon acquainted me with the whole Matter: We very carefully examined them. The *Polypi* were very nearly of the Colour of the Buff formed on the Surface of highly pleuritic or rheumatic Blood, when quite cold, or rather whiter. They were vastly tough, and seemed to be formed of various *Lamina* very closely connected, though here-and-there a bloody Vein, as it were, was interspersed.—They were not only firmly attached to the fleshy *Columnæ* of the Heart, but were also sunk and inserted strongly into the *Intercolumnia*, or *Sulci*, and that even to the very Bottom of the *Ventricules*.—These Roots, if we may so call them, were of a whiter Colour than the Body of the *Polypus*.

One of these *Polypi* (taken out of the Heart of *Jeremy Mannings*) weighed a full Ounce, not including its Ramifications in the *Arteria Pulmonaris* and the *Cava*, but as it was taken out of the Right Atricle and Ventricle; for it was one continued Mass, and strongly adhered to both.

The *Polypus* taken out of the Left Ventricle of the same Heart, was also very considerable, and rather more firm and compact than that of the Right, but of the very same Colour, and firmly implanted into the Sides of the Ventricle quite down to the *Micro Cordis*.—Its Branches were shot a great Way into the Subclavian and Carotid Arteries—but very little
down

down the *Aorta*.—I observed one of the semilunar Valves of the *Aorta* beginning to grow bony.

There were likewise found very great *Polypi* in the Right and Left Cavities of the other Heart, of the same Colour, Firmness and Tenacity, but not altogether so large; and they respectively branched their *Appendices* a great Way into the Pulmonary Artery, *Aorta*, &c.

More of the Sailors dying in the very same way soon after, the *Thorax* of another was opened, that of a young Man about Twenty.—In the Right Auricle and Ventricle of his Heart was found a large tough subrubicund *Polypus*, not quite so white as those mentioned before—but there was no such Concretion in the Left.

Now though *Kerkringius* and others have endeavoured to explode the Notion of the Formation of true *Polypi* in the Heart and Blood-vessels; yet *Malpighius*, *Bartholine*, *Tulpius*, *Pechlin*, and others, have given us incontestable Instances of the Existence of true *Polypi* in the Heart, in the strictest Sense; and you have here Three unquestionable Evidences of the like Nature: Such, indeed, especially the Two former, as I never have before met with amidst the very numerous Dissections I have been first and last present at.

Before I conclude this, it may not be amiss to mention, that I had the first Lieutenant and Purser of the *Dunkirk* under my Care in very severe *Pleuro-peripneumonies*, whose Blood was as viscid as I ever saw; and they were with very great Difficulty saved, nor could they be brought to expectorate till the Seventh Day of the Fever.

It may be observed also, that the above Ships came Home from a very hot Climate into a very cold one, in the midst of Winter, and that a long-continued Course of North-easterly Winds kept on, and even increased, the Cold to a great Degree—that Pleurifies, *Peripneumonies*, &c. are commonly the Effects of such a Constitution of Air—that the Blood of such as labour under these Disorders is always extremely fizy; and that the Heat of the Weather in the *West-Indies*, and large and long-continued Use of spirituous Liquors, had greatly condensed the Blood of these poor Fellows; and that, in the Blood-vessels of the *Thorax* of such as die of these Disorders, *polypose* Concretions are not uncommonly found.

I am, Sir, with the greatest Respect,

Plymouth,
April 23. 1742.

*Your obliged,
and most obedient,
humble Servant,*

J. Huxham.

VII. *An Extract of a Topographical* Account of Bridgnorth in the County of Salop, communicated to the ROYAL SOCIETY by the Rev. Mr. Stackhouse, Minister of St. Mary Magdalen in that Town; containing an Account of the Situation, Soil, Air, Births and Burials of that Place, and of some Tumuli Sepulchrales near it.*

Read June 3.
1742.

Bridgnorth^a is pleasantly situated upon the River *Severn*, on the West of the antient Forest of *Morfe*, and was built, according to *Camden*, by *Edelfleda*^b, Lady of the *Mer-*

* Taken from the original Papers of the Rev. Mr. *Richard Cornes*, late Minister of the Parish of *St. Mary Magdalen* in *Bridgnorth*.

^a A softer Pronunciation only of its old Name *Brugg* or *Brugg-north*. In a Charter of King *John*, it is called *Bruges*; in another of *Edward III.* *Brugg* and *Brugg-north*, and in a Third of King *Charles I.* *Bridgnorth*, alias *Brugg-north*, alias *Bruges*. Both *Brugg* and *Bruges* signify a Bridge or Bridges, and the Termination *North*, whether it be, as some would have it, a Corruption of the Word *Morfe* or not, was doubtless added with regard to the Situation of the Place. *N. B.* *Bruges* in *Flanders* is so called from its many Bridges, and *Brugg-bote* is an old Word for Pontage or Bridge-toll.

^b *Edelfleda* alias *Elfleda*, eldest Daughter of *Alfred the Great*, said by some to be the first absolute Monarch over the *English*. She married *Ethelred*, to whom *Alfred* gave the Government of the City of *London*, which he had then taken from the *Danes*, and the Title of Earl of *Mercia*, an empty Title, till by his Valour he became Master of a great Part of that Province. After his Death, *Elfleda*, being a Princess of great martial Prowess, took upon her the Government of her Husband, and fortified many Towns, to keep the *Danes* out of *Mercia*:

Mercians; but encompassed with a Wall, and fortified, by *Robert de Belesme* ^c, Earl of *Shrewsbury*; and afterwards favoured by King *John*, and other Kings, with many and great Privileges granted in their respective Charters. It is governed by Two Bailiffs with the Burgessees in Common Hall assembled: The Bailiffs are annually chosen out of Twenty-four Aldermen upon St. *Matthew's* Day, after the following remarkable Manner: The Court being met, the Names of Twelve Aldermen (Seniors of those that are there present, and who have not been Callers for three Years before) being separately written upon small Scrolls of Paper, all of the same Size, and rolled up close by the Town-Clerk, are thrown into a large Purse, which being shut, well shaken and tossed by the two Chamberlains, standing upon the Chequer ^d, is afterwards held open betwixt them, before the Bailiffs; whence each Bailiff, according to Seniority, putting in his Hand, takes a Scroll, by which the Callers are fixed, who immediately mounting the Chequer, alternately call the Jury out of such

Mercia: Afterwards she carried her Arms, in Conjunction with her Brother King *Edward*, against the *Welsh*, and obliged them to become tributary. About this time (913) she is said to have built and repaired several Places, as *Stafford*, *Bridgnorth*, &c. See *Rapin* from *Sax. Ann Hunting. Howend* Vol. I pag. 38.

^c *Robert de Belesme*, a Man outrageously cruel to his own Sons and

prisoned, others say,
 fitment for his notorious Wickedness. See *Camden*, *Baker's Chronicle*, &c.

^d A large square Table in the Middle of the Court, encompassed with Seats.

Persons

Persons as are Burgesſes, and then preſent in Court, to the Number of Fourteen. Theſe being all ſworn neither to eat nor drink, till they, or Twelve of them, have made Choice of Two fit Perſons (who have not been Bailiffs for Three Years before) to ſerve the Office of Bailiffs for the Year enſuing, are locked up together, until agreed; which hath often occaſioned very long and tedious Faſtings, even to the Prejudice of their Healths: However, when they are agreed, they make Report of the Perſons they have elected, and they are ſworn into Office upon *Michaelmas-day*.

This Borough, as others, has a Recorder, Town-Clerk, and Two Representatives in Parliament.

The Town is divided by a ſtately Stone Bridge^f over the *Severn* into Two unequal Parts; the leſſer Part, that lies upon the Eaſt of the River, is called the low Town, and conſiſts of Two Streets, one extending from the Bridge to the very Foot of *Morfe*, and goes by the Name of *St. John's-ſtreet*, from a Religious Houſe there in Times of Popery, dedicated to *St. John* the-Baptiſt.

The River abounds with divers ſorts of the moſt excellent Fiſh, as *Salmon*, *Pike*, *Shad*, *Trout*, *Greyling*, *Flounders*, *Eels*, *Chub*, *Gudgeon*, and what goes here by the Name of *Samlet*, a ſmall Fiſh ſpotted with Red, not much unlike the *Trout*; only the Spots

^e The Bailiffs for the Time being are Juſtices of the Peace, and Lords of the Manor for the ſaid Town and Liberties, which are extenſive, being one way Six or Seven Miles.

^f This Bridge has Seven Arches, and formerly had a Draw, Portcullis, and other Engines of Defence: The old Gate-houſe upon it is ſtill ſtanding, and ſeveral other Houſes have been built upon its Piers.

lie in a more direct Line on its Sides. It seldom exceeds Four or Five Inches in Length, and is of a most delicious Taste, but to be taken only at certain Seasons of the Year : In Summer, when the Water is low, the Fisher goes bare-legged into the Shallows, and, having on a Pair of old Shoes, flirs up the Gravel and Sand, so as to discolour the Water; and thus, by angling there, usually takes many of them, together with *Gudgeons* and *Blays*; but they are mostly taken with an artificial Fly.

The Head of this River is on the Mountain *Plymlymon* in the County of *Montgomery*, whence it flows through this County, that of *Worcester*, and *Gloucester*, diffusing its vital Moisture as it passes, till it empties itself into the *Severn* Sea below the City of *Bristol*. It is navigable for about 140 Miles, and has a great Number of Vessels continually plying upon it.

The Soil in these Parts is of a very different Nature: Eastward of the River *Severn* lies a fine dry sandy Soil, fit for bearing *Rye*, *Barley*, &c. and is therefore commonly distinguished by the Name of the *Rye-land* from the other Parts of the Country, that lie on the West of the River; where the Soil is much upon a moist Clay, fit for *Wheat*, *Pease*, &c. yet not so peculiarly adapted to these sorts of Grain, but that several lighter Parts of this Quarter oftentimes bear very plentiful Crops of *Barley*, *Oats*, &c.

The Common Fields adjoining to the Town bear Grain of all Kinds, one of them being yearly ap-

* Most of the Vessels made use of upon this River are built here in several Dock-yards.

propriated for Corn; nay, the very Sides of the Rock upon which the Town stands, though the Soil there be but shallow, yet, when well manured, produces great and very early Crops of *Pease*, *Beans*, *Cucumbers*, *Asparagus*, and all sorts of Garden-herbs in Perfection.

The high Town lies upon the Western Bank of the River: That rises gradually to a considerable Height. The Ascent begins from the End of the Bridge, where what is first worth Notice, is, a Passage ^h for People on Foot, cut deep in the Rock, ascending with convenient Flights of Steps at proper Distances, much resembling, as Travellers have observed, the Ascent of Mount *Calvary* in *Jerusalem*. On the South of this Passage, opens a large Cave ⁱ in the Rock, remarkable here for being the Repository of excellent Beer: At the Entrance of this stands a Lion rampant, carved in Stone, and within is a large Tun containing above Five Hogsheads.

The Air of this Place is exceeding healthy, and, for ought I know, may vie even with that of *Montpelier* itself. It is certain, we have very few consumptive People amongst us, so that as it is preferative to the Natives, in all Probability it might be restorative to Strangers. However, we have this Convenience from the Variety of Situation ^k, that if

^h About 160 Yards in Length.

ⁱ In Length 33, in Breadth 27 Feet.

^k Dr. *Hollins*, an eminent Physician in *Shrewsbury*, Father to the late Dr. *Hollins*, Physician to his present MAJESTY, made it his Observation, That when any Epidemical Distempers were abroad, *Bridgnorth* was sooner freed from them than any other Place that he knew. The same hath been since confirmed by the Observations of Dr. *Antony Weaver*, now an ingenious Physician in this Place.

the Air in the upper Part of the Town be too fine and sharp for our Constitutions, we may soon remove into the Lower, where it is much softer, and by that means possibly find Relief, and continue till Old Age in its natural Course carries us to the Grave. In short, many of the Inhabitants here live to very advanced Years, there being many Instances of those that have exceeded an Hundred¹.

¹ N.B. There are three old Hatters now living (1739) in the Parish of *St. Mary Magdalen*, and bidding fair for an Hundred each, whose present Ages, being computed together, make somewhat more than 257 Years.

A Table of Births and Burials for 12 Years, in the Parish of St. Mary Magdalen, which contains about 520 Families; and of St. Leonard, containing about 550 Families; which, allowing Five to each Family, amounts to 2600 Inhabitants in the Parish of St. Mary, and to 2750 in the Parish of St. Leonard; in all 5350.

In the Parish of
St. Mary Magdalen.

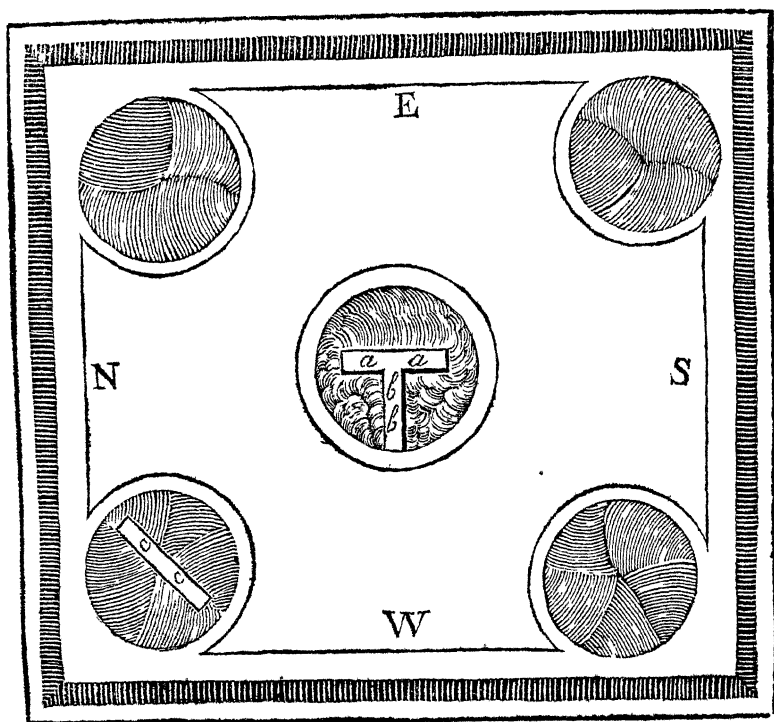
Births.	Burials.	
54.	119.	Sm. Pox. 1727.
72.	77. 1728.
52.	74. 1729.
65.	78. 1730.
75.	36. 1731.
64.	41. 1732.
70.	46. 1733.
69.	77. 1734.
46.	56. 1735.
60.	32. 1736.
67.	22. 1737.
61.	53. 1738.
755.	711.	

In the Parish of
St. Leonard.

Births.	Burials.	
68.	100.	Sm. Pox.
72.	61.	
54.	78.	
84.	65.	
70.	53.	
47.	49.	
79.	65.	
64.	90.	
72.	57.	
79.	39.	
71.	56.	
62.	65.	
822.	778.	

Total Increase 88.

Sepulchral Tumuli upon Morfe near Bridgnorth, in the County of Salop, supposed to be Danish.



East and West Sides
36 Yards long.

North and South Sides
33 Yards long.

In *July* 1740. I observed upon *Morfe* the *Tumuli* as represented above, where the Soil is a strong Gravel. *Montfaucon* in his *Antiquities* tells us, that the old *Cimbri*^m were wont to throw up Heaps of Gra-

^m Old Inhabitants of *Denmark*.

vel upon their Graves; and that the more remarkable the Persons were, the larger were the *Tumuli* over them. I therefore imagined, that this might possibly be a Burying-place of the *Danes*, who, I think, 'tis generally owned, were Descendants of those People. For Satisfaction, I caused the middle and largest *Tumulus* to be dug through from North to South (See *aa* in the Figure), supposing by that Method I must cross the Site of any Body that might have been laid there. We dug about Seven Feet deep, even to the solid Rock, without meeting with any thing remarkable, but an iron Shell, in Shape of a small Egg, with a round Hole at one End, but so cankered and decayed, that it easily broke into small Pieces; this we judged to have been the Bos of a Sword. However, upon viewing the Trench that we had dug, we perceived upon the West Side a Hollow in the Gravel, which, upon Trial, extended horizontally Four or Five Feet; and under this Hollow (See *bb* in the Figure) we found one of the large *Vertebra* of the Loins, with its Processes pretty perfect, but thoroughly petrified; and, upon further Search, several Portions of Bones, all alike petrified, but so disguised, that we could not discover to what Part of the Body they belonged. We afterwards opened one of the lesser *Tumuli* (See *cc* in the Figure), and found what is thought to be the *Os Sacrum*, and many other small Pieces of Bones, in a petrified State. It was great Odds that we had found nothing at all; but Nature favoured us, by preserving some few Tokens of Antiquity. During this Search, the People were much alarmed, and flocked to the Place in great Numbers, expecting, I presume, to have seen Wonders;

but

but being disappointed, they soon spread a Report over the Country, that by a Discovery made by some ancient Writings; we dug there for Treasure, by which we were greatly enriched: To prevent the further Concourse of the People, &c. we were glad to fill up the Trenches; and leave the other *Tumuli* unexamined.

N. B. The Middle *Tumulus* is about Nine Yards in Diameter, and the lesser about Eight Yards each at the Plain.

VIII. *Part of a Letter from the Right Hon^{ble} Robert James Lord Petre, F. R. S. to Martin Folkes, Esq; P. R. S. concerning some extraordinary Effects of Lightning.*

S I R, *Brook-street, June 24. 1742.*

Read June 24.
1742.

ON *Tuesday* Morning, between Three and Four o'Clock, we had at *Thorndon* some of the most terrible Thunder I ever heard; and, indeed, by the Effects of it, I have Reason to conclude, that it was very near us, as well as by the Noise, to which I really think no other Thunder I ever yet had any Notion of, could be compared. It has beat down a Chimney at a Farm-house just by, and the Lightning has also struck Two large Oaks in my Park, which stand about Forty or Fifty Feet apart. In one of them I do not observe any thing much different from other Trees which I have before seen struck with Lightning; the only thing that seems remarkable, is, that the greatest Damage ap-
pears

pears to be done to the East Side of the Tree, although it is certain, that the Storm all came from the South-West. This Tree is extremely shattered, and split from the Top to the Bottom; and on the South-West Side, just by the Root, there is a large Hole made in the Ground, about Six or Seven Inches Diameter, and about a Foot or Fifteen Inches deep. But in the other Tree, I think, there is something more particular; for there, without shattering or splitting the Tree in the least, or so much as disturbing a single Branch, although there are a great many upon it, the Lightning has taken off the Bark about Five Inches wide, in a complete spiral Line, from about Forty Feet high, down to within about a Foot of the Ground, where the Width diminishes to about Two Inches, and so goes quite off: In the Centre of these Five Inches, it has entered the Wood about Three-fourths of an Inch deep, and about an Inch and half wide: This Hollow it has in great part cleared out intirely, and the rest is left hanging like Pieces of broken or untwisted Ropes; this Hollow also diminishes near the Ground, and dies quite out exactly at the Ground: The spiral Line is exactly regular, and goes just once round the Tree, or but very little more, and, as near as I can observe, is exactly of an equal Width all the Way. The Surface of the Bark of both the Trees is remarkably touched for about Ten Feet from the Ground, as if it were shot all over with Small-shot, each of which seems to have taken off little Scales or outside Pieces of the Bark, from an Inch and half or Two Inches broad and long, to a quarter of an Inch. I will add no more, than only to wish, that this Account may prove
 any

any thing new or agreeable to you, to desire you to excuse the great Hurry in which it is wrote, and to assure you, that I am very sincerely,

SIR,

Your faithful

Humble Servant,

Petre.

N. B. Effects of Lightning, like this, were observed by Sir John Clark. See *Philosophical Transactions*, N^o 454. p. 235.

IX. *An Account of a Meteor seen at Peckham,*
Dec. 11. 1741. *by Tho. Milner, M. D.*

Read June 24.
1742.

DEcember 11. 1741. at Seven Minutes past One in the Afternoon by the common Clocks, a Globe of Light, somewhat larger than the horizontal Full Moon, and as bright as the Moon appears at any time while the Sun is above the Horizon, instantaneously appeared, in a blue clear Sky, about the S. S. E. moving towards the East with a continual equable Motion, and leaving behind it a narrow Streak of Light, whiter than the Globe itself, throughout its whole Course. Towards the End it appeared less than at the Beginning of its Motion; and within Three, or at most Four Seconds, it suddenly vanished. Its apparent Velocity was nearly equal to half the Velocity of those usual Meteors commonly called falling or shooting Stars: This may be thought an indeterminate way of expressing its Velocity, as those falling Stars vary in the Swiftness of their Motions; but if such be understood as have

a mean Velocity, between the swiftest and the slowest, it expresses, in the best manner I can think of, the apparent Velocity of its Motion.

The narrow luminous Streak remained very distinct after the Globe was gone; and gave a fair Opportunity for taking the Elevation of this *Phænomenon* above the Horizon, at the Beginning and End of its Motion, &c. had there been proper Instruments ready at hand: This not being the Case, I guessed the Elevation of the Globe, when it first appeared, was near 30° . But some Days after, being exactly in the same Situation as when I saw this Meteor, I took the Elevation of a small Cloud, which appeared to be in the same Place, with a Quadrant of Two Feet Radius, and found it to be but 20° . This luminous Tract, or Path, seemed a Right Line, not quite parallel, but a little inclined to the Plane of the Horizon, *viz.* highest towards the East. It was at first very narrow, and pointed at each Extremity; but soon grew broader, and within 20 Minutes after the Appearance, which was the last time I saw any thing of this Affair, it appeared exactly like a long bright rare Cloud, discontinued in two Places, above three times its first Breadth, and a little more inclined to, and elevated above the Horizon, than it was immediately after the Motion of the Globe.

Peckham,
June 20. 1742.

Thomas Milner.

X. *Some Conjectures concerning Electricity, and the Rise of Vapours, by J. T. Desaguliers, LL. D. F. R. S.*

Read June 24.
1742.

IT is proper first to mention by way of Preliminary, That *Monsr. Du Faye's* Assertion of Two Sorts of Electricity is found to be true by Observations and Experiments, *viz.* That Bodies endowed with the Vitreous Electricity repel one another, and attract those that have the Resinous Electricity; on the contrary, those that are endowed with the Resinous Electricity repel one another, but attract those that have the Vitreous Electricity.

I suppose Particles of pure Air to be Electric Bodies always in a State of Electricity, and that Vitreous Electricity.

1st, Because Particles of Air repel one another without touching, as has been deduced from Experiments and Observations.

2^{dly}, Because, when the Air is dry, the Glass Tube rubbed (or only warmed) throws out its *Effluvia*, which the Air drives back to the Tube, from whence they dart out anew, and so move backwards and forwards with a vibratory Motion, which continues their Electricity.

3^{dly}, Because the Feather made electric by the Tube, and darted from it, keeps its Electricity a long time in dry Air; whereas, when the Air is moist, the moist Particles, which are non-electric, being attracted by the Feather, soon make it lose this Electricity, which also happens even to the Tube in a little time.

From

From this Consideration it will be easy to account for a famous Experiment of the late Mr. *Hawksbee*, which is this—

Having pumped out all the Air from a Glass Globe, he caused it to turn on its Axis very swiftly, by means of a Rope with a Wheel and Pully; then rubbing the Glass with his Hand during its Motion, there appeared a great deal of Light of a purple Colour within the Globe, without any Light or Attraction observed on the Outside of the Glass, which is observed when the Air has not been pumped out. Then turning the Cock so as to readmit the Air gently into the Globe during its Motion, the Light was broken and interrupted, diminishing gradually, till at last it appeared only on the Outside of the Glass, where it was accompanied with Attraction. Does it not appear to be, that at first the external Air by its Resistance drives back the Electric *Effluvia*, which go then to the Inside of the Globe, where there is the least Resistance? For we observe, that as the Air comes in, it repels the Electric *Effluvia*, that go inwards no longer, when all the Air is come in. If the Fact be so, as the Experiment shews, is not my Conjecture proved, *viz.* that the Air is Electrical?

In the Reverend and Learned Dr. *Hales's Vegetable Statics*, several of his Experiments shew, that Air is absorbed, and loses its Elasticity by the Mixture of sulphureous Vapours, so that Four Quarts of Air in a Glass Vessel will be reduced to Three. Will not this *Phenomenon* be explained by the different Electricity of Sulphur and Air? The *Effluvia* of Sulphur, being electric, repel one another; and the Particles of Air, being also electric, do likewise repel each

other. But the Air being electrical of a Vitreous Electricity, and Sulphur of a Resinous Electricity, the Particles of Air attract those of Sulphur, and the *Molecule* compounded of them, becoming non-electric, lose their repulsive Force.

It has for a great while been thought, that watery Vapours, that rise in Air to form Clouds, used to rise, because the Water which is of itself specifically heavier than Air, (being formed into little hollow Spherules or Bubbles filled with an *Aura*, or thinner Air than the ambient Air) in this new State made a Fluid of little Shells, specifically lighter than the ambient Air in which it must rise. But Philosophers are come off of that Opinion; and such as have implicitly come into it, may find it refuted in the *Philosophical Transactions*, N^o 407.

Now may not this *Phænomenon* of the Rise of Vapours depend upon Electricity in the following manner?

The Air which flows at Top of the Surface of the Waters is electrical, and so much the more as the Weather is hotter. Now in the same manner as small Particles of Water jump towards the electric Tube, may not those Particles jump towards the Particles of Air, which have much more specific Gravity than very small Particles of Water, and adhere to them? Then the Air in Motion having carried off the Particles of Water, and driving them away as soon as it has made them electrical, they repel one another, and also the Particles of Air. This is the Reason that a cubic Inch of Vapour is lighter than a cubic Inch of Air; which would not happen, if the Particles of Vapour were only carried off in the Interstices

terfices of Air, because then a cubic Inch of Air, loaded with Vapour, would be made specifically heavier than an Inch of dry Air; which is contrary to Experiments, which shew us by the Barometer, that Air which is moist, or full of Vapours, is always lighter than dry Air.

XI. *An Account of Margaret Cutting, a young Woman, now living at Wickham Market in Suffolk, who speaks readily and intelligibly, though she has lost her Tongue.*

Read July 1.
1742.

A Brief Account of this young Woman's Case, in a Letter from Mr. *Benjamin Boddington*, of *Ipswich*, *Turkey-Merchant*, to Mr. *Henry Baker*, F.R.S. was communicated to the ROYAL SOCIETY in the Month of *February* last, and appeared so extraordinary, that Mr. *Baker* was desired to make all possible Inquiries into the Reality of the Fact, and lay before the SOCIETY what Information he should receive in relation thereto.

In pursuance of this, he wrote to Mr. *Boddington*, and begged the Favour of him to make the strictest and most critical Inquiry he was able into this Affair, not only by viewing the young Woman's Mouth, and examining her himself, but also by calling to his Assistance some skilful Gentleman in the Physical Way, and any other learned and judicious Person whom he might judge most likely to contribute towards discovering the real Truth, and detecting any Error, Fallacy, or Imposition. He likewise desired they

they would heedfully observe her Manner of speaking and articulating the Sounds of those Letters and Syllables, in the Formation whereof the *Apex* of the Tongue seems more particularly needful: And, in order to render their Examination more easy, as well as more satisfactory, he sent a List of Letters and Sounds, together with several such Sentences as he imagined would be most difficult to be pronounced without the Help of the Tongue.

Mr. *Boddington*, as soon after this as their Affairs would give them Leave, prevailed upon Mr. *Notcutt*, a Minister, a learned and curious Gentleman, and Mr. *Hammond*, who perfectly understands Anatomy, to accompany him to *Wickham Market*, about Twelve Miles from *Ipswich*, where the young Woman lives; whose Case (after they had inspected her Mouth, and examined her in the strictest Manner) is set forth in the following Certificate signed by them all.

Ipswich, April 9. 1742.

WE have this Day been at *Wickham Market*, to satisfy our Curiosity concerning *Margaret Cutting*, a young Woman, who, we were informed, could talk and discourse without a Tongue.

She informed us, that she was now more than Twenty Years of Age, born at *Turnstal*, a Village within Four Miles of *Wickham Market* in *Suffolk*, where she lost her Tongue by a Cancer [being then about Four Years old]. It first appeared like a small black Speck on the upper Superficies of the Tongue, and soon eat its Way quite to the Root of it. She was under the Care of Mr. *Scotchmore*, a Surgeon of *Saxmundham*, who soon pronounced the Case incurable:
How-

However, he continued using the best Means he could for her Relief. One Day when he was Syringing of it, the Tongue dropped out, and they received it into a Plate, the Girl, to their Amazement, saying to her Mother, " Don't be frightened, Mamma ; 'twill grow " again." It was near a Quarter of a Year after, before it was quite cured.

We proceeded to examine her Mouth with the greatest Exactness we could, but found not the least Appearance of any remaining Part of a Tongue, nor was there any *Uvula*. We observed a fleshy Excrecence on the under Left Jaw, extending itself almost to the Place where the *Uvula* should be, about a Finger broad : This Excrecence, she said, did not begin to grow till some Years after the Cure : It is by no means moveable, but quite fixed to the Parts adjacent. The Passage down the Throat, at the Place where the *Uvula* should be, or a little to the Right of it, is a circular open Hole, large enough to admit a small Nutmeg.

Notwithstanding the want of so necessary an Organ as the Tongue was generally supposed to be, to form a great Part of our Speech, and likewise to be assisting in Deglutition, to our great Admiration, she performed the Office of Deglutition, both in swallowing Solids and Fluids, as well as we could, and in the same manner : And as to Speech, she discoursed as fluently and well as other Persons do ; though we observed a small Sound, like what is usually called speaking through the Nose ; but, she said, she had then a great Cold, and she believed that occasioned it. She pronounced Letters and Syllables very articulately ; the Vowels she pronounced perfectly, as also
those

those Consonants, Syllables, and Words, that seemed necessary to require the Help of the Tongue, as * *d, l, t, n, r, at, al, ath, ash, cha, la, ta, ja.* The little Dog did not eat Bread.—Touch the Tooth.—Try to light the Candle.—Thrice Thirty-three.—Let the large Cat scratch the little Dog.—The Church.—doth.—Lilly.— All these she pronounced perfectly. She read to us in a Book very distinctly and plain; only we observed, that sometimes she pronounced words ending in *ath* as *et*—*end* as *emb*—*ad* as *eib*—; but it required a nice and strict Attention to observe even this Difference of Sound. She sings very prettily, and pronounced her Words in Singing as is common. What is still very wonderful, notwithstanding the Loss of this useful Organ the Tongue, which is generally allowed by Anatomists, and Natural Philosophers, to be the chief, if not the sole Organ of Taste, she distinguishes all Tastes very nicely, and can tell the least perceivable Difference in either Smell or Taste.

We the Underwritten do attest the above to be a true Account.

Benjamin Boddington.

William Notcutt, Minister.

William Hammond, Apothecary.

Mr. *Baker* received along with the foregoing Certificate, by Letter from Mr. *Boddington*, some farther Particulars, which he supposed less material.—He says, in her Person she is a little thin Body, genteel

* These were the Letters, Sounds and Sentences mentioned by Mr. *Baker*.

enough, a pretty good Face, fair Complexion, with light-brown Hair, of a weakly Constitution, lame on one Side, through Weakness after a Fever and the Small-pox, which she had last Summer. She seems a well-behaved Girl, and has nothing of a Country Mien. She discourses agreeably, very fluently and pertinently, has every thing clean and neat about her, gets her Livelihood by making Mantuas, and has an Aunt in *London*, named *Mary Cutting*, who is House-keeper to the Dowager Lady *Rockford* in *Bond-street*.

He says, if she were among Twenty People in a Room, he thinks it would be impossible for a Stranger by any means to guess her being the Person without a Tongue, for she has no odd Motion of her Mouth or Lips in Speaking: She sings with an easy Air, and modulates her Voice prettily.—He asked her, if she did not miss her Tongue, or find any Inconvenience from the Want of it? She answered, No: Not in the least; nor could she imagine what Advantage he had in the Use of his. He inquired, how she did to guide her Food in her Mouth to eat: She replied, very easily, she could eat before, on one Side or the other as she pleased, but could not explain the Manner how. He was very observing to see her eat, but could discern no Difference from others in the moving of her Jaws, or other Motions of her Face, nor in her swallowing Food, or in drinking; she did both very neatly, and had exactly the same Motion in her Throat as we have in its passing down.

He was apprehensive the Excrescence mentioned in the Certificate, might, in some measure, supply the Use of a Tongue; but she assured him, it never

moved in the least, and that she spoke as well before it began to grow (which was several Years after the Cure); and Mr. *Hammond* convinced him, by trying with their Fingers and a Spoon, that it was quite fixt and immoveable.—He observes further, that she is no ways assisted by a good Set of Teeth; for she has but few, those bad, and scarce so high as her Gums.—He asked her, in what Part of her Mouth her most sensible Taste lay? She said, it was all over alike; and, smiling, added, She was afraid she was too nice in that; for, if her Butter was not curious, she eat dry Bread.

Mr. *Boddington*, in another Letter to Mr. *James Theobald*, F. R. S. dated the 14th of *April* 1742. after giving an Account of this young Woman in the Manner as before, adds, He can recollect nothing more, except her telling him, that though she was able to speak from the very first losing of her Tongue, she was not so happy as to her Deglutition; for she was unable to swallow any thing solid for many Months after, without its being minced very fine, and then thrust into her Throat by a Finger: But by degrees, she knows not how, she became able to manage without that Help, and could eat any thing in the same manner as other Persons can.—He adds, That, in his own Mind, he thinks the fleshy Excrecence is of great Service to her, though she cannot make out in what manner: That for his own part, he had formerly supposed it as impossible to speak without a Tongue, as to see without Eyes; and therefore expects many, who shall hear this Account, will continue Unbelievers, and think he and his Friends are all mistaken, that they do not know what they see, and that

that their Ignorance is the only Ground of their Admiration.

While Mr. *Baker* was making his Inquiries, he was informed, that Mr. *John Dennis*, Tobacconist, in *Aldersgate-street*, could give him a full and satisfactory Account of this Affair: He therefore applied to Mr. *Dennis*, who assured him in a very civil, candid and intelligent Manner, that he was well acquainted with *Margaret Cutting*, having many Years ago been carried by a Gentleman to see her as a Prodigy for being able to speak without a Tongue: That he had seen her several times since, commonly calling on her when he travels that Way, and carrying some Friend or other with him; and at all these times he had inspected her Mouth, and was sure she had no Tongue: And that last Summer, in particular, he and another went to see her: That he would declare this under his Hand, and should always be ready to attest the Truth of it to any Body, or in any Manner. He likewise gave an Account how she lost her Tongue, as he had it from her Mother, who died some Years ago, and it was exactly as above related; and said he had been told the same by an Apothecary also, who had her in Hand along with Dr. *Scotchmore*.

The Testimony of Mr. *Dennis*, and the Person who saw her with him last Summer, is as follows:

March 20. 1741.

WE the under-written saw *Margaret Cutting*, at *Wickham Market* in *Suffolk*, in or about *June* last; and, examining her Mouth, found she had no Tongue, and yet she speaks very intelligibly.

John Dennis.
Gabriel Daniells.

MYself saw her in about Two or Three Years after her Tongue was lost, had a full Account of it from her Mother, heard her then speak, and have seen and heard her divers times since, and heard her talk better and better.

She was under the Care of Dr. *Scotchmore* at *Saxmundham, Suffolk*.

John Dennis.

Mr. *Dennis* (upon Mr. *Baker's* Inquiry) wrote to the young Woman herself, acquainting her, that many People would not believe it possible for her to speak without a Tongue, and desiring she would not be ashamed to give an Account of herself under her own Hand; in Answer to which he received the following Letter :

To Mr. John Dennis, in Aldersgate-street.

Sir,
THIS being the first Opportunity that I had to answer your Letter, I assure you, that I have no more Tongue in my Mouth than I had when you saw me last, which is none; but, Thanks be to my God, I have had the Happiness to speak ever since it came out, which was when I was about Four Years old. As for my Age now, I cannot rightly tell, but I think I am about Twenty-four Years old. I would have none suspect the Truth of it; for I have no Tongue, and can speak very well, and this is from my own Hand. I was not ashamed to write about myself, but of my bad Writing. So no more, but I am

Your humble Servant,

Margaret Cutting.

The

The Case of this young Woman is indeed extraordinary *; but there are several Examples of like Nature to be met with in medical Writers, and those of the greatest Authority; one of which, as it has the Attestation of a whole University, cannot be improper to mention here. *Monsieur Drelincourt*, a very noted Physician, tells us, in his Treatise on the Small-pox, of a Child Eight Years of Age, who had lost his Tongue by that Distemper, and was yet able to speak, to the Astonishment of the University of *Saumur* in *France*; and that the University (who doubtless had first carefully examined into the Truth) had attested it, by drawing up a particular Account of the Fact, that Posterity might have no room to doubt concerning the Validity of it. The Account is to be met with at large, in the Third Volume of the *Ephemerides Germanicæ*, under the Title of *Aglossostomographia*.

Tulpius too makes mention of a Man who had the Misfortune to have his Tongue cut out by the *Turks*, and yet, after Three Years, could speak very distinctly. He says, he went himself to *Ivesop*, a Town in *Holland*, to be satisfied of the Truth of it, and found it to be as it was reported. Nay, he does not so much as mention any Defect in his Speech, but assures us, that he could pronounce those Letters which depend upon the *Apex* of the Tongue, even the Consonants, very articulately. And this Case is still the more worthy Attention, because the Patient could not swallow even the least Quantity of Food, unless he thrust it into the *Oesophagus* by means of his Finger.

* N. B. All the original Papers are in the Repository of the *Royal Society*.

If we go back to earlier Times, the Emperor *Justin*, in *Cod. Tit. de Off. Præf. Præt. Af.* says, he had seen venerable Men, *qui abscissis radicitus Linguis, pœnas miserabiliter loquebantur, whose Tongues having been cut out by the Roots, they miserably spoke*, or complained, of the *Punishments* they had suffered. And again, *Nonnullos alios, quibus Honorichius Vandalorum Rex Linguas radicitus præciderat, loquelam tamen habuisse integram*, that some others, whose Tongues *Honorichius*, King of the *Vandals*, had cut out by the Roots, yet perfectly retained their Speech.

XII. *A remarkable Conformation, or Lusus Naturæ, in a Child; by C. Warwick, Surgeon, in Truro, Cornwall.*

Read July 1.
1742.

ABOUT April 1741. one *Sarah Allen*, of the Parish of *St. Blazy*, near *Truro*, having been married near Four Years, and Mother of Two Children, well-formed and living, was brought to-bed of my present Subject, but of so remarkable and preternatural a Constitution, as must render its whole Life inevitably miserable, the Particulars whereof, from my repeated Observations, are as follows:

The *Umbilicus* is nearly in its natural Site, but somewhat large and prominent, having more the Appearance of a Tumour, than the ordinary irregular Shape of that Organ.

Immediately below this *Umbilicus*, is a large fungous Excrecence, nearly the Size of a small Egg, but somewhat

somewhat depressed, of a fiery Aspect, and exquisitely sensible. The Surface is irregular, being composed of divers Granulations or small Lobes of Flesh; and the Basis of it I could not well discover, my Endeavours being attended with much Pain and Difficulty; however, from the branchy Top of it, I am inclined to think it somewhat pendulous.

Beneath, adjoining to this *Fungus*, is another pretty large Excrescence, neither sensible nor spongy, as the former, but of a solid uniform Contexture. Its Projection from the *Abdomen* is about One-third of an Inch, and, was there a Section made parallel to its Basis, it would be of an Elliptical Figure. In Shape and Dimensions it somewhat resembles the *Glans Penis*, its Surface being covered with the same fine *Membrane*, and has a small Indenture in the Top of it, but it is not so large, and has no Aperture in it.

Suspended to this *Glans*, like the *Omentum* to the *Ventricule*, is a large *Membrane* of a semilunar Figure, loose, flexible, and, when turned up, capable of covering some Part of it. Its Texture nearly resembles that of the *Præputium*, or is somewhat thicker. There is likewise a small Cord or *Frænum*, which arising from the Circumference of this *Membrane*, and bisecting the above *Glans*, terminates under the *Fungus*. About half an Inch below this *Membrane*, is a wrinkled Extuberance resembling a *Scrotum*, but of an uncertain Magnitude, great or small, as the Descent of the Infant's *Intestines*, which having broken their natural Confines, form an unseemly Roll from one *Inguen* to the other. Its Situation is about the upper Edge of the *Os Pubis*, which, in examining this Part, I found greatly deficient, and I am apt to believe, from

from the great Chasm which I perceived there, it must be intirely wanting.

The next thing to be observed is the *Anus*. I found the Situation of this Part more forward than usual, at least by Two Inches; and, if my Conjectures be right, the *Rectum*, from this Position, must take its Course nearly through the Chasm of the *Os Pubis*.

Besides all these Inconveniencies, to complete the Child's Misery, there is a perpetual Distillation of Urine from some unseen Passages under the *Fungus*, exciting by its Acrimony, every Moment, Pains and Excoriations.

To conclude : Its Sex is so imperfect, and obscurely represented, that it received no Baptism till Four Months after it was born; when its Parents, flattering themselves that Nature might take a Turn some time or other for the Child's Advantage, gave it an Appellation applicable to either Sex, as Time and Circumstances should require.

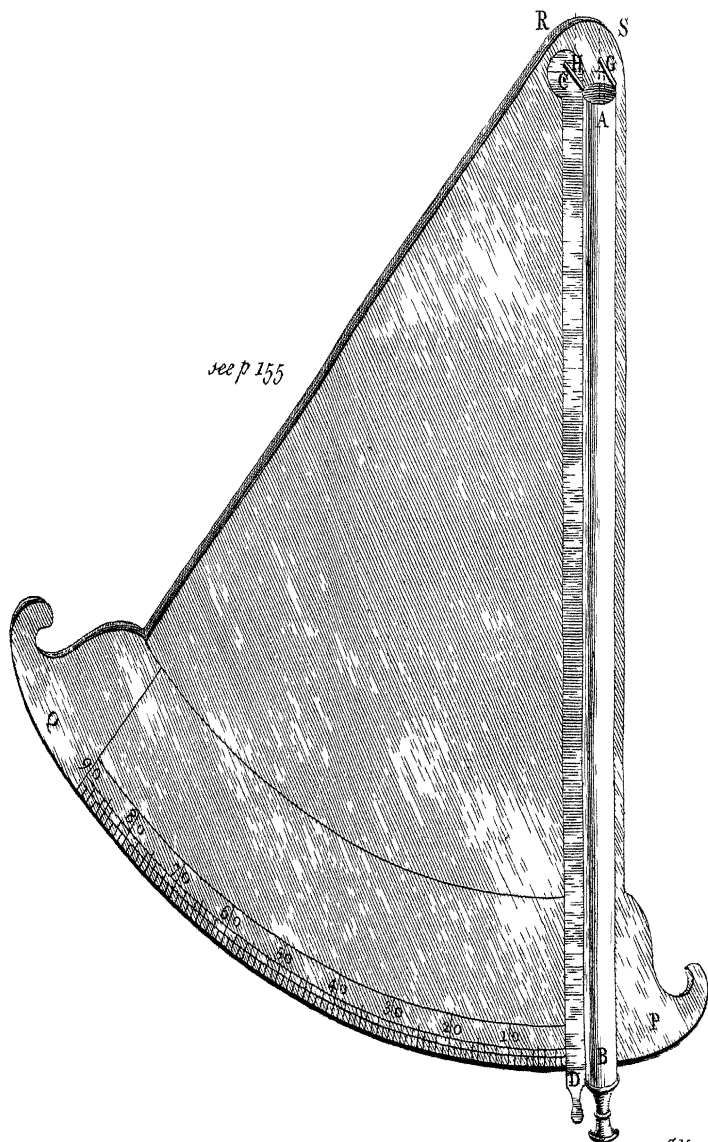
Truro, April 21. 1742.

Explanation of the Figure prefixed.

<i>A. Umbilicus.</i>	<i>D. Glans.</i>	<i>G. Anus.</i>
<i>B. Fungus.</i>	<i>E. Membrana.</i>	<i>H. Frænum.</i>
<i>CC. Prolapsus.</i>	<i>F. Scrotum.</i>	

July 1. 1742. The SOCIETY adjourned to October 28.

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetstreet*; and C. DAVIS, over-against *Gray's Inn Gate* in *Holbourn*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.



PHILOSOPHICAL TRANSACTIONS.

For the Month of *October* and part of *November*,
1742.

The CONTENTS.

- I. *A true Copy of a Paper found, in the Hand Writing of Sir Isaac Newton, among the Papers of the late Dr. Halley, containing a Description of an Instrument for observing the Moon's Distance from the Fixt Stars at Sea.* Page 155.
- II. *The Effects of Cold; together with Observations of the Longitude, Latitude, and Declination of the Magnetic Needle, at Prince of Wales's Fort, upon Churchill-River in Hudson's Bay, North America; by Capt. Christopher Middleton, F.R.S. Commander of His MAJESTY'S Ship Furnace, 1741-2.* 157.
- III. *The Report of the Committee of the ROYAL SOCIETY appointed to examine some Questions in Gunnery.* 172.
- IV. *An*

The CONTENTS.

- IV. *An Account of a Meteor seen near Holkam in Norfolk, in August 1741. transmitted to the ROYAL SOCIETY by the Right Hon^{ble} Thomas Lord Lovell, F. R. S.* 183.
- V. *An Account of the Proportions of the English and French Measures and Weights, from the Standards of the same, kept at the ROYAL SOCIETY.* 185.
- VI. *A Method of making a Gold-colour'd Glazing for Earthen-Ware; communicated in a Letter, in Latin, from M. Godofridus Heinſius, Aſtron. Prof. at St. Petersburgh, to Mr. Peter Collinſon, F. R. S.* 188.

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetſtreet*; and C. DAVIS, over-againſt *Gray's-Inn-Gate* in *Holbourn*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.

- I. *A true Copy of a Paper found, in the Hand Writing of Sir Isaac Newton, among the Papers of the late Dr. Halley, containing a Description of an Instrument for observing the Moon's Distance from the Fixt Stars at Sea.*

*Read at a
Meeting of the
Royal Society,
October 28.
1742.*

IN the annexed Scheme, *PQRS* denotes a Plate of Brass, accurately divided in the Limb *DQ*, into $\frac{1}{2}$ Degrees, $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, by a Diagonal Scale; and the $\frac{1}{2}$ Degrees, and $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, counted for Degrees, Minutes, and $\frac{1}{6}$ Minutes.

AB, is a Telescope, three or four Feet long, fixt on the Edge of that Brass Plate.

G, is a *Speculum*, fixt on the said Brass Plate perpendicularly, as near as may be to the Object-glass of the Telescope, so as to be inclined 45 Degrees to the Axis of the Telescope, and intercept half the Light which would otherwise come through the Telescope to the Eye.

CD, is a moveable Index, turning about the Centre *C*, and, with its fiducial Edge, shewing the Degrees, Minutes, and $\frac{1}{6}$ Minutes, on the Limb of the Brass Plate *PQ*; the Centre *C*, must be over-against the Middle of the *Speculum G*.

H, is another *Speculum*, parallel to the former, when the fiducial Edge of the Index falls on *oo^d oo'* *oo''*; so that the same Star may then appear through
X the

the Telescope, in one and the same Place, both by the direct Rays and by the reflex'd ones ; but if the Index be turned, the Star shall appear in two Places, whose Distance is shewed, on the Brass Limb, by the Index.

By this Instrument, the Distance of the Moon from any Fixt Star is thus observed : View the Star through the Perspicil by the direct Light, and the Moon by the Reflex (or on the contrary) ; and turn the Index till the Star touch the Limb of the Moon, and the Index shall shew upon the Brass Limb of the Instrument, the Distance of the Star from the Limb of the Moon ; and though the Instrument shake, by the Motion of your Ship at Sea, yet the Moon and Star will move together, as if they did really touch one another in the Heavens ; so that an Observation may be made as exactly at Sea as at Land.

And by the same Instrument, may be observed, exactly, the Altitudes of the Moon and Stars, by bringing them to the Horizon ; and thereby the Latitude, and Times of Observations, may be determined more exactly than by the Ways now in Use.

In the Time of the Observation, if the Instrument move angularly about the Axis of the Telescope, the Star will move in a Tangent of the Moon's Limb, or of the Horizon ; but the Observation may notwithstanding be made exactly, by noting when the Line, described by the Star, is a Tangent to the Moon's Limb, or to the Horizon.

To make the Instrument useful, the Telescope ought to take in a large Angle : And to make the Observation true, let the Star touch the Moon's Limb, not on the Outside of the Limb, but on the Inside.

II. *The Effects of Cold; together with Observations of the Longitude, Latitude, and Declination of the Magnetic Needle, at Prince of Wales's Fort, upon Churchill-River in Hudson's Bay, North America; by Capt. Christopher Middleton, F. R. S. Commander of His MAJESTY's Ship Furnace, 1741-2.*

Read OCT. 28.
1742.

I Observed, that the *Hares, Rabbits, Foxes and Partridges*, in *September*, and the Beginning of *October*, changed their native Colour to a snowy White; and that for Six Months, in the severest Part of the Winter, I never saw any but what were all white, except some *Foxes* of a different Sort, which were grizzled, and some half red, half white.

That Lakes and standing Waters, which are not above 10 or 12 Feet deep, are frozen to the Ground in Winter, and the Fishes therein all perish.

Yet in Rivers near the Sea, and Lakes of a greater Depth than 10 or 12 Feet, Fishes are caught all the Winter, by cutting Holes through the Ice down to the Water, and therein putting Lines and Hooks. But if they are to be taken with Nets, they cut several Holes in a strait Line the Length of the Net, and pass the Net, with a Stick fastened to the Head-line, from Hole to Hole, till it reaches the utmost Extent; and what Fishes come to these Holes for Air, are thereby entangled in the Net; and these Fish, as soon

as brought into the open Air, are instantaneously frozen as stiff as Stock-fish. The Seamen likewise freshen their salt Provisions, by cutting a large Hole through the Ice in the Stream or Tide of the River, which they do at the Beginning of the Winter, and keep it open all that Season. In this Hole they put their salt Meat, and the Minute it is immersed under Water, it becomes pliable and soft, though before its Immersion it was hard frozen.

Beef, Pork, Mutton, and Venison, that are killed at the Beginning of the Winter, are preserved by the Frost, for Six or Seven Months, intirely free from Putrefaction, and prove tolerable good eating. Likewise *Geese, Partridges*, and other Fowl, that are killed at the same time, and kept with their Feathers on, and Guts in, require no other Preservative but the Frost to make them good wholesome eating, as long as the Winter continues. All kinds of Fish are preserved in the like manner.

In large Lakes and Rivers, the Ice is sometimes broken by imprisoned Vapours; and the Rocks, Trees, Joists and Rafters of our Buildings, are burst with a Noise not less terrible than the firing off a great many Guns together. The Rocks which are split by the Frost, are heaved up in great Heaps, leaving large Cavities behind; which I take to be caused by imprisoned watery Vapours, that require more Room, when frozen, than they occupy in their fluid State. Neither do I think it unaccountable, that the Frost should be able to tear up Rocks and Trees, and split the Beams of our Houses, when I consider the great Force and Elasticity thereof. If Beer or Water is left in Mugs, Cans, Bottles, nay in Copper Pots, though they

they were put by our Bed-sides, in a severe Night they are surely split to pieces before Morning, not being able to withstand the expansive Force of the inclosed Ice.

The Air is filled with innumerable Particles of Ice, very sharp and angular, and plainly perceptible to the naked Eye. I have several times this Winter tried to make Observations of some celestial Bodies, particularly the Emerfions of the Satellites of *Jupiter*, with reflecting and refracting Telescopes; but the Metals and Glasses, by that Time I could fix them to the Object, were covered a quarter of an Inch thick with Ice, and thereby the Object rendered indistinct, so that it is not without great Difficulties that any Observations can be taken.

Bottles of *strong Beer, Brandy, strong Brine, Spirits of Wine*, set out in the open Air for Three or Four Hours, freeze to solid Ice. I have tried to get the Sun's Refraction here to every Degree above the Horizon, with *Elton's* Quadrant, but to no Purpose, for the Spirits froze almost as soon as brought into open Air.

The Frost is never out of the Ground, how deep we cannot be certain. We have dug down 10 or 12 Feet, and found the Earth hard frozen in the Two Summer Months; and what Moisture we find Five or Six Feet down, is white like Ice.

The Waters or Rivers near the Sea, where the Current of the Tide flows strong, do not freeze above Nine or Ten Feet deep.

All the Water we use for Cooking, Brewing, &c. is melted Snow and Ice; no Spring is yet found free from freezing, though dug never so deep down. All

Waters

Waters in-land are frozen fast by the Beginning of *October*, and continue so till the Middle of *May*.

The Walls of the House we live in are of Stone, Two Feet thick, the Windows very small, with thick wooden Shutters, which are close shut 18 Hours every Day in the Winter. There are Cellars under the House, wherein we put our *Wines, Brandy, strong Beer, Butter, Cheese, &c.* Four large Fires are made in great Stoves, built on purpose, every Day: As soon as the Wood is burnt down to a Coal, the Tops. of the Chimneys are close stopped with an Iron Cover: This keeps the Heat within the House (though at the same time the Smoke makes our Heads ake, and is very offensive and unwholsome); notwithstanding which, in Four or Five Hours after the Fire is out, the Inside of the Walls of our House and Bed-places will be Two or Three Inches thick with Ice, which is every Morning cut away with a Hatchet. Three or Four times a Day we make Iron Shot of 24 Pounds Weight red-hot, and hang them up in the Windows of our Apartments. I have a good Fire in my Room the major Part of the 24 Hours, yet all this will not preserve my *Beer, Wine, Ink, &c.* from freezing.

For our Winter Dress we make use of Three Pair of Socks of coarse Blanketting or Duffield for the Feet, with a Pair of *Deer-skin* Shoes over them; Two Pair of thick *English* Stockings, and a Pair of Cloth Stockings upon them; Breeches lined with Flannel; Two or Three *English* Jackets, and a Fur or Leather Gown over them; a large Beaver Cap, double, to come over the Face and Shoulders, and a Cloth of Blanketting under the Chin; with Yarn Gloves, and

a large Pair of Beaver Mittings hanging down from the Shoulders before, to put our Hands in, which reach up as high as our Elbows; yet notwithstanding this warm Cloathing, almost every Day, some of the Men that stir abroad, if any Wind blows from the Northward, are dreadfully frozen; some have their Arms, Hands and Face blistered and frozen in a terrible manner, the Skin coming off soon after they enter a warm House, and some have lost their Toes. Now their lying-in for the Cure of these frozen Parts, brings on the Scurvy in a lamentable manner. Many have died of it, and few are free from that Distemper. I have procured them all the Helps I could, from the Diet this Country affords in Winter, such as fresh Fish, Partridges, Broths, &c. and the Doctors have used their utmost Skill in vain; for I find nothing will prevent that Distemper from being mortal, but Exercise and stirring abroad.

Coronæ and *Parhelia*, commonly called *Halo's*; and *Mock-Suns*, appear frequently about the Sun and Moon here. They are seen once or twice a Week about the Sun, and once or twice a Month about the Moon, for Four or Five Months in the Winter, several *Coronæ* of different Diameters appearing at the same time.

I have seen Five or Six parallel *Coronæ* concentric with the Sun several times in the Winter, being for the most part very bright, and always attended with *Parhelia* or *Mock-Suns*. The *Parhelia* are always accompanied with *Coronæ*, if the Weather is clear; and continue for several Days together, from the Sun's Rising to his Setting. These Rings are of various Colours, and about 40 or 50 Degrees in Diameter.

The

The frequent Appearance of these *Phænomena* in this frozen Climate seems to confirm *Descartes's* Hypothesis, who supposes them to proceed from Ice suspended in the Air.

The *Aurora Borealis* is much oftener seen here than in *England*; seldom a Night passes in the Winter free from their Appearance. They shine with a surprising Brightness, darkening all the Stars and Planets, and covering the whole Hemisphere: Their tremulous Motion from all Parts, the Beauty and Lustre, are much the same as in the Northern Parts of *Scotland* and *Denmark*, &c.

The dreadful long Winters here may almost be compared to the Polar Parts, where the Absence of the Sun continues for Six Months; the Air being perpetually chilled and frozen by the Northerly Winds in Winter, and the cold Fogs and Mists obstructing the Sun's Beams in the short Summer we have here; for notwithstanding the Snow and Ice is then dissolved in the Low-lands and Plains, yet the Mountains are perpetually covered with Snow, and incredible large Bodies of Ice continue in the adjacent Seas. If the Air blows from the Southern Parts, the Air is tolerably warm, but very cold when it comes from the Northward, and it seldom blows otherwise than between the North-east and North-west, except in the Two Summer Months, when we have, for the major Part, light Gales between the East and the North, and Calms.

The Northerly Winds being so extremely cold, is owing to the Neighbourhood of high Mountains, whose Tops are perpetually covered with Snow, which exceedingly chills the Air passing over them.

The

The Fogs and Mists that are brought here from the Polar Parts, in Winter, appear visible to the naked Eye in Icicles innumerable, as small as fine Hairs or Threads, and pointed as sharp as Needles. These Icicles lodge in our Cloaths, and if our Faces or Hands be uncovered, they presently raise Blisters as white as a Linen Cloth, and as hard as Horn. Yet if we immediately turn our Backs to the Weather, and can bear our Hand out of our Mittin, and with it rub the blistered Part for a small time, we sometimes bring the Skin to its former State: If not, we make the best of our Way to a Fire, and get warm Water, where-with we bathe it, and thereby dissipate the Humours raised by the frozen Air; otherwise the Skin would be off in a short time, with much hot, serous, watry Matter coming from under along with the Skin; and this happens to some almost every time they go abroad for Five or Six Months in the Winter, so extreme cold is the Air when the Wind blows any thing strong.

Now I have observed, that when it has been extreme hard Frost by the Thermometer, and little or no Wind that Day, the Cold has not near so sensibly affected us, as when the Thermometer has shewed much less freezing, having a brisk Gale of Northerly Wind at the same time. This Difference may perhaps be occasioned by those sharp-pointed Icicles before-mentioned striking more forcibly in a windy Day, than in calm Weather, thereby penetrating the naked Skin, or Parts but thinly covered, and causing an acute Sensation of Pain or Cold: And the same Reason, I think, will hold good in other Places; for should the Wind blow Northerly any thing hard for

many Days together in *England*, the Icicles that would be brought from the Polar Parts by the Continuance of such a Wind, though imperceptible to the naked Eye, would more sensibly affect the naked Skin, or Parts but slightly covered, than when the Thermometer has shewn a greater Degree of freezing, and there has been little or no Wind at the same time.

It is not a little surprising to many, that such extreme Cold should be felt in these Parts of *America*, more than in Places of the same Latitude on the Coast of *Norway*; but the Difference I take to be occasioned by Wind blowing constantly here, for Seven Months in the Twelve, between the North-east and North-west, and passing over a large Tract of Land, and exceeding high Mountains, &c. as before-mentioned. Whereas at *Drunton* in *Norway*, as I observed some Years ago in wintering there, the Wind all the Winter comes from the North and North North-west, and crosses a great Part of the Ocean clear of those large Bodies of Ice we find here perpetually. At this Place we have constantly every Year Nine Months Frost and Snow, and unsufferable Cold from *October* till the Beginning of *May*. In the long Winter, as the Air becomes less ponderous towards the Polar Parts, and nearer to an *Equilibrium*, as it happens about One Day in a Week, we then have Calms and light Airs all round the Compass, continuing sometimes 24 Hours, and then back to its old Place again, in the same manner as it happens every Night in the *West-Indies*, near some of the Islands.

The Snow that falls here is as fine as Dust, but never any Hail, except at the Beginning and End of Winter. Almost every Full and Change of the Moon, very hard Gales from the North.

The constant Trade Winds in these Northern Parts I think undoubtedly to proceed from the same Principle, which our learned Dr. *Halley* conceives to be the Cause of the Trade Winds near the Equator, and their Variations.

“ Wind, says he, is most properly defined to be
 “ the Stream or Current of the Air; and where such
 “ Current is perpetual and fixed in its Course, it is
 “ necessary, that it proceed from a permanent and
 “ unintermitting Cause, capable of producing a like
 “ constant Effect, and agreeable to the known Pro-
 “ perties of Air and Water, and the Laws of Motion
 “ of fluid Bodies. Such an one is, I conceive, the
 “ Action of the Sun’s Beams upon the Air and Wa-
 “ ter, as he passes every Day over the Oceans, con-
 “ sidered together with the Nature of the Soil and
 “ Situation of the adjoining Continents. I say,
 “ therefore, first, That according to the Laws of
 “ *Statics*, the Air which is less rarefied or expanded
 “ by Heat, and consequently more ponderous, must
 “ have a Motion towards those Parts thereof which
 “ are more rarefied, and less ponderous, to bring it to
 “ an *Equilibrium*, &c.”

Now, that the cold dense Air, by reason of its great Gravity, continually presses from the Polar Parts towards the Equator, where the Air is more rarefied, to preserve an *Equilibrium* or Balance of the Atmosphere, I think, is very evident from the Wind in those frozen Regions blowing from the North and

North west, from the Beginning of *October* until *May*; for we find, that when the Sun, at the Beginning of *June*, has warmed those Countries to the Northward, then the South east, East and variable Winds continue till *October* again; and I do not doubt but the Trade Winds and hard Gales may be found in the Southern Polar Parts to blow towards the Equator, when the Sun is in the Northern Signs, from the same Principle.

The Limit of these Winds from the Polar Parts, towards the Equator, is seldom known to reach beyond the 30th Degree of Latitude; and the nearer they approach to that Limit, the shorter is the Continuance of those Winds. In *New-England* it blows from the North near Four Months in the Winter; at *Canada*, about Five Months; at the *Danes* Settlement in *Streights Davis*, in the 63d Degree of Latitude, near Seven Months; on the Coast of *Norway*, in 64, not above Five Months and a half, by reason of blowing over a great Part of the Ocean, as was before-mentioned; for those Northerly Winds continue a longer or shorter Space of Time, according to the Air's being more or less rarefied, which may very probably be altered several Degrees, by the Nature of the Soil, and the Situation of the adjoining Continents.

The vast Bodies of Ice we meet with in our Passage from *England* to *Hudson's-Bay*, are very surprising, not only as to Quantity, but Magnitude, and as unaccountable how they are formed of so great a Bulk, some of them being immersed 100 Fathom or more under the Surface of the Ocean; and a Fifth or Sixth Part above, and Three or Four Miles in Circumference.

cumference. Some Hundreds of these we sometimes see in our Voyage here, all in Sight at once, if the Weather is clear. Some of them are frequently seen on the Coasts and Banks of *Newfoundland* and *New-England*, though much diminished.

When I have been becalmed in *Hudson's-Streights* for Three or Four Tides together, I have taken my Boat, and laid close to the Side of one of them, founded, and found 100 Fathom Water all round it. The Tide floweth here above Four Fathom; and I have observed, by Marks upon a Body of Ice, the Tide to rise and fall that Difference, which was a Certainty of its being aground. Likewise, in a Harbour in the Island of *Resolution*, where I continued Four Days, Three of these Isles of Ice (as we call them) came aground. I founded along by the Side of one of them, quite round it, and found 32 Fathom Water, and the Height above the Surface but 10 Yards; another was 28 Fathom under, and the perpendicular Height but Nine Yards above the Water.

I can in no other manner account for the Aggregation of such large Bodies of Ice but this: All along the Coasts of *Streights Davis*, both Sides of *Baffin's-Bay*, *Hudson's-Streights*, *Anticosti*, or *Labradore*, the Land is very high and bold, and 100 Fathoms, or more, close to the Shore. These Shores have many Inlets or Fairs, the Cavities of which are filled up with Ice and Snow, by the almost perpetual Winters there, and frozen to the Ground, increasing for Four, Five, or Seven Years, till a kind of Deluge or Land-flood, which commonly happens in that Space of Time throughout those Parts, breaks them loose, and launches them into the Streights or Ocean, where they
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are driven about by the variable Winds and Currents in the Months of *June*, *July*, and *August*, rather increasing than diminishing in Bulk, being surrounded (except in Four or Five Points of the Compass) with smaller Ice for many Hundred Leagues, and Land covered all the Year with Snow, the Weather being extreme cold, for the most part, in those Summer Months. The smaller Ice that almost fills the Streights and Bays, and covers many Leagues out into the Ocean along the Coast, is from Four to Ten Fathom thick, and chills the Air to that Degree, that there is a constant Increase to the large Isles by the Sea's washing against them, and ~~the perpetual~~ wet Fogs, like small Rain, freezing as they settle upon the Ice; and their being so deeply immersed under Water, and such a small Part above, prevents the Wind's having much Power to move them: For though it blows from the North-west Quarter near Nine Months in Twelve, and consequently those Isles are driven towards a warmer Climate, yet the progressive Motion is so slow, that it must take up many Years before they can get Five or Six hundred Leagues to the Southward; I am of Opinion some Hundreds of Years are required; for they cannot, I think, dissolve before they come between the 50th and 40th Degree of Latitude, where the Heat of the Sun consuming the upper Parts, they lighten and waste in Time: Yet there is a perpetual Supply from the Northern Parts, which will so continue as long as it pleases the AUTHOR of all Beings to keep things in their present State.

Ob-

*Observations of the Longitude, Latitude, and
the Declination of the Magnetic Needle, at
Prince of Wales's Fort, Churchill-River.*

Having observed the apparent Time
of an Emerſion of *Jupiter's* firſt Sa- } h. ' ''
tellite at *Fort-Churchill*, on *Saturday* } 11 55 50
the 20th of *March* laſt 1741-2. at

I find the ſame Emerſion happened
at *London*, by Mr. *Pound's* Tables,
compared with ſome Emerſions actually } 18 15 10
obſerved in *England* near the ſame, at

Whence the hoary Difference of
Meridians, between *Fort-Churchill* and } 6 19 20
London, comes out

Which converted into Degrees of
the Equator, gives for the Diſtance of } 94° 50'
the ſame Meridians

Wherefore, ſince the Time at *London* was later in
Denomination than that at *Churchill*, it follows that,
according to this Obſervation, *Churchill* is 94 De-
grees 50 Minutes, in Longitude Weſt of *London*.

I took ſeveral other Obſervations, which agreed
one with another to leſs than a Minute, but this I
look upon as the moſt diſtinct and beſt.

The Obſervation was made with a good. 15 Foot
refracting Telescope, and a Two Foot Reſlector of
Gregory's Kind, having a good Watch of Mr. *Gra-*
ham's that I could depend upon; for I have frequent
Opportunities of diſcovering how much its Vari-
ation amounted to, and conſtantly found its daily
De-

Deviation or Error to be 15 Seconds too slow; by which means it was as useful to me for all Purposes, as if it had gone most constantly true without any Change. This Watch I kept in my Fob in the Day, and in Bed in the Night, to preserve it from the Severity of the Weather; for I observed, that all other Watches were spoiled by the extreme Cold.

I have found, from repeated Observations, a Method of obtaining the true Time of the Day at Sea, by taking Eight or Ten different Altitudes of the Sun or Stars, when near the Prime Vertical, by Mr. *Smith's* or Mr. *Hadley's* Quadrant, which I have practised these Three or Four Years past, and never found from the Calculations, that they differed one from another more than 10 or 15 Seconds of Time. This Certainty of the true Time at Sea is of greater Use in the Practice of Navigation, than may appear at first Sight; for you thereby not only get the Variation of the Compass without the Help of Altitudes, but likewise the Variation of the Needle from the true Meridian, every time the Sun or Star is seen to transit the same. Also having the true Time of Day or Night, you may be sure of the Meridian Altitude of the Sun or Star, if you get a Sight 15 or 20 Minutes before or after it passes the Meridian; and the Latitude may be obtained to less than Five Minutes, with several other Uses in Astronomical Observations; as the Refraction of the Atmosphere, and to allow for it, by getting the Sun's apparent Rising and Setting, which any body is capable of doing, and from thence you will have the Refraction.

If we had such a Telescope contrived as Mr. *Smith* recommends to be used on Shipboard at Sea, now we can have an exact Knowledge of the true Time of
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the Day or Night from the above Instruments and a good Watch, I hope we should be able to observe the Eclipses of the first *Satellite* of *Jupiter*, or any other *Phænomenon* of the like Kind, and thereby find the Distance of Meridians, or Longitude at Sea.

The Variation of the Magnetical Needle, or Sea-Compass, observed by me at *Churchill* in 1725. (as in N^o 393. of the *Philosophical Transactions* for the Months of *March* and *April* 1726.) was at that Time North 21 Degrees Westerly, and this Winter I have carefully observed it at the same Place, and find it no more than 17 Degrees, so that it has differed about One Degree in Four Years; for in 1738. I observed it here, and found its Declination 18 Degrees Westerly. I have carefully observed, and made proper Allowance for the Sun's Declination and Refraction, and find the Latitude here to be 58 Degrees 56 Minutes North: But in most Parts of the World, where the Latitudes are fixed by Seamen, they are for the most part falsely laid down, for want of having regard to the Variation of the Sun's Declination, which, computed at a distant Meridian, when the Sun is near the Equator, may make a great Error in the Sun's rising and setting *Azimuths*, &c.

These things I thought proper to take Notice of, as they may be of Service to Navigators, and the Curious in Natural Inquiries.

The foregoing Relation having been given by Capt. *Middleton* to the late worthy President of the ROYAL SOCIETY, Sir *Hans Sloane*, Bart. he was pleased to communicate the same to the SOCIETY, and at the same time, as the surviving Trustee of the late Sir *Godfrey Copley*, to nominate Capt. *Middleton* to receive this Year the Prize Medal, given annually by the ROYAL SOCIETY, in Consequence of Sir *Godfrey's* Benefaction; and the same was accordingly presented to the Captain on St. *Andrew's* Day last, 1742.

III. *The Report of the Committee of the ROYAL SOCIETY appointed to examine some Questions in Gunnery.*

Read Nov. 4.
1742.

DR. *Furin* having proposed * Two Questions in Gunnery to be examined, the SOCIETY was pleased to appoint a Committee for that Purpose.

The Questions were,

1. *Whether all the Powder of the Charge be fired, before the Bullet is sensibly moved from its Place?*
2. *Whether the Distance to which the Bullet is thrown, may not become greater or less, by changing the Form of the Chamber, though the Charge of Powder and all other Circumstances continue unchanged?*

At the Meeting of the Committee it was proposed to divide the First Question into Two Parts.

1. *Whether all the Powder of the Charge be fired?*
2. *Whether all the Powder that is fired, be fired before the Bullet is sensibly moved from its Place?*

As to the First Part of the First Question, the Committee are of Opinion, that all the Powder of the Charge is not fired.

They found their Opinion upon the following Experiments:

Pieces of Paper used for Hangings were laid close together upon the Ground, to the Breadth of Ten Feet, in the Line of a Fowling-piece, between it and a Frame of Ten Feet square, covered over with Paper. Upon pointing the Piece towards the Middle of the

* June 24. 1742.

Frame, and *discharging* it several times *with* and *without* Ball, some Powder was always collected, but mixed with a great deal of Dirt.

It is however to be observed, that in Two Experiments made the 22d of *July*, near the Artillery-Ground, before the President and some of the Fellows of the SOCIETY, with a finer sort of Powder, in a Barrel of Three Feet Nine Inches in Length, and Three-fourths of an Inch Bore, with Twelve Penny-weight of Powder the First time, and Twenty-four Penny-weight the Second time, without Ball or Wadding, no Powder could be found scattered on the Paper laid before the Piece, nor sticking to a Board at the Distance of about Ten Feet, against which the Piece was pointed. But when the same Powder was fired in a short Barrel of Five Inches Two-tenths of an Inch in the Chace, either with or without Ball, some Quantity of Powder was always collected.

Other Experiments were afterwards made before the Committee, by firing a Fowling-piece charged with Five Penny-weight of Powder, against a Sheet of whited-brown Paper, at the Distance of Two or Three Yards; the Paper was found pierced with several Hundred Holes, and the Jags of the Paper appeared on the Backside. In a second Trial with Ten Penny-weight, the Paper had more Holes in it. A third Trial was made with Five Penny-weight of Powder and Ball, and then few Holes appeared in the Paper. In a fourth Experiment made with a short Screw-barrel Pistol, with a Charge of One Penny-weight two Grains of Powder and a Ball, several Holes were found in the Paper *.

* That the Paper in these Experiments was pierced by the unfired Powder, appears, because several Grains were found lying behind the Frame, to which the Paper was fixed, and some few stuck in the Paper.

But the Irregularities in this manner of collecting the Powder unfired, giving reason to suspect, that some Powder escaped Sideways, beyond the Paper laid to receive it, it was proposed to have a Machine made, which being close every-where but at the End where the Muzzle of the Piece was to be placed, might thereby hinder the Powder from being dissipated. Such a Machine was contrived by Mr. *Ellicot*, and by him presented to the Committee, being a Frame of Wood in Shape like a truncated quadrangular Pyramid; at the smaller End was a Board to receive the Shot, and the Four Sides of the Machine were covered with thick Paper strongly pasted together, and so prepared as to prevent its taking Fire. This Machine, supported by Props, was placed upon one of its Angles, the Carriage for fixing the Barrels was placed close to the greater Base, which was left open. The Result of the several Experiments were as follows:

The Three first Experiments were made with a Barrel Eight-tenths of an Inch Diameter of the Bore, and the Length of the Chace Five Inches Two-tenths of an Inch. The Charge each time was Six Penny-weight of Powder without Ball; the Quantities of Powder collected were respectively, One Penny-weight Nineteen Grains; One Penny-weight Twenty-one Grains; and One Penny-weight Twenty Grains.

Three other Experiments were made with the same Piece, and with Twelve Penny-weight Charge, without Ball. The Quantities of Powder collected were Four Penny-weight Eighteen Grains; Four Penny-weight Two Grains and an half; and Four Penny-weight Twenty-two Grains.

The next Three Trials were with the same Piece, the Charge Six Penny-weight, with a Ball weighing One Ounce Four Penny-weight, being a Mixture of Lead and Tin, and fitting the Piece exactly.

The Quantities of Powder collected each time were respectively, One Penny-weight Five Grains; One Penny-weight Five Grains; and One Penny-weight Eleven Grains.

The last Three Experiments with the same Piece, were made with a Charge of Twelve Penny-weight, the Weight of the Ball as before; and the Quantities of Powder collected, were found to be One Penny-weight Twelve Grains; One Penny-weight Nine Grains; and One Penny-weight Eight Grains and an half.

The Waddings used in all these and the following Experiments, were of thick Leather cut round, to fit the Bore of the Piece.

The Committee then proceeded to examine what Alteration might arise from a greater *Length of Chace*. The Experiments in this Case were made with a Barrel Three Foot Nine Inches in Length, and Three-fourths of an Inch in the Bore; the Charges of Powder, and Weight of leaden Balls, were as before.

In the First Three Experiments with Six Penny-weight Charge, without Ball, the Quantities of Powder collected were Three Grains; Nine Grains; and Nine Grains, respectively. In the Three next Experiments, with Twelve Penny-weight Charge, without Ball, the Quantities of Powder collected were Thirteen Grains; Nine Grains; and Sixteen Grains and an half. The Three following Experiments were with Six Penny-weight Charge and a Ball. The Powder collected was Two Grains; Three Grains; and Two Grains.

The

The last Experiments were made with Twelve Penny-weight Charge and Ball, as before; the Quantities of Powder collected from Two Discharges were respectively, Two Grains; and Four Grains and an half. The Frame being broke, a third Experiment could not be made.

The Powder collected after the several Discharges, was put into separate Boxes; it seemed much bruised, and mixed with Dirt. Yet several of the Parcels being tried, fired with brisk Explosions; and some of the Powder collected from the Experiments with the short Barrel, amounting to Six Penny-weight Sixteen Grains, being put into the long Barrel, and fired with Ball, went off with a strong Report; and the Ball pierced the Deal-board, at the End of the Frame, and penetrated Two Inches deep into an Elm-plank placed to receive the Balls.

Some Gentlemen, present at these Experiments, suspecting that Part of the Powder might escape at the open End of the Frame; the short Barrel was fired with Twelve Penny-weight of Powder and Ball, as before; through a very large Funnel, the Quantities found, after Three Discharges, were severally, One Penny-weight Two Grains; Sixteen Grains; and Fifteen Grains.

Whereas upon removing the Funnel, and discharging the Piece, as before, One Penny-weight Eleven Grains was collected, agreeably to former Experiments; it seems that the Funnel had a like Effect as lengthening the Piece.

Some Experiments were also made with the short Barrel, filled up with Lead, so as to leave but Three Inches and Three-fourths of an Inch for the Chace, the Piece
being

being then charged with Twelve Penny-weight of Powder and Ball, as before; the Surface of the Ball was but Eight-tenths of an Inch within the Mouth of the Piece, and the Powder collected, after Three Discharges, was respectively, Two Penny-weight Two Grains; One Penny-weight Seventeen Grains; and One Penny-weight Eleven Grains.

The Barrel being further filled up, so as to leave but Two Inches Eight-tenths for the Chace, and charged as before, the Ball rising about One-fifth of an Inch beyond the Mouth of the Piece, the Powder collected, after the Discharge, was Two Penny-weight Six Grains. Upon a Second Trial, the Ball being as much within the Mouth, One Penny-weight Sixteen Grains was collected. And at the Third Trial, the Ball being level with the Mouth, Two Penny-weight Six Grains were again found.

The Committee also caused some Experiments to be made of the Effect of a *Touch-hole* near the Fore-part of the Charge. They found upon discharging the short Piece of Five Two-tenths of an Inch Chace, the Charge Twelve Penny-weight and Ball, as before, the Touch-hole being near the Fore-part of the Powder; the Quantities of Powder, severally collected, were One Penny-weight Seven Grains and an half; One Penny-weight Six Grains; and One Penny-weight Four Grains. And upon a Discharge made with a little more Powder, which filled the Barrel exactly to the Edge of another Touch-hole, the former being screwed up, the Quantity collected was One Penny-weight Nine Grains.

The Effect of firing with *heavy Slugs* was also examined: The Weight of the Slugs and Quantities of

of Powder collected, were as follows; the Charge in the short Barrel being Twelve Penny-weight :

Discharge.	Weight of Slugs.			Powder collected.	
	Ounces.	dwt.	gr.	dwt.	gr.
I.	2.	13.	0.	1.	3.
II.	2.	11.	14.	0.	17.
III.	2.	12.	0.	0.	8.
IV.	5.	5.	6.	0.	13.
V.	5.	3.	0.	0.	8 $\frac{1}{2}$.

The Powder used in all these Experiments, made before the Committee, was presented to them by Mr. *Walton*, and is such as he makes for the King's Service. To ascertain as nearly as possible, that the Powder had not undergone any considerable Alteration by Damps or otherwise, a Standard Experiment was previously made at every Meeting, with the short Barrel charged with Twelve Penny-weight of Powder, and with a Ball of Twenty-four Penny-weight; and the Quantity of Powder collected was from One Penny-weight Eight Grains, to One Penny-weight Twelve Grains; which is as great a Regularity as can well be expected. This Powder of Mr. *Walton* being *sifted*, and divided into a *fine* and a *large Sort*, the following Discharges were made with Twelve Penny-weight of each, and Ball as usual:

Discharges with fine Powder.	Powder collected.	
	dwt.	gr.
I.	1.	4.
II.	0.	21.
III.	0.	12.

In this Third Experiment the Bullet, not being so exactly turned as the others, was rammed down with great Force.

Dis-

Discharge with large Powder.	Powder collected.
	<i>dwt.</i> <i>gr.</i>
I.	I. 11.
II.	I. 16.
III.	I. 21.

And the Powder being bruised in a Mortar, and sifted through a Lawn Sieve, the Charge and Ball being as before, what was collected after Three Discharges, was One Penny-weight Ten Grains, One Penny-weight Eight Grains, and Seventeen Grains.

Mr. *Watson* having had two Parcels of Powder delivered to him, the one fresh, and the other collected after Discharges with Ball, gave an Account of the Quantity of Nitre he had separated from them, *viz.*

Separated from Nine Penny-weight of fresh Powder	} dwt. gr.	
Nitre		6. 2.
Residuum		2. 7.

Loss 0. 15.

From Nine Penny-weight of Powder collected after having been discharged with Ball	} dwt. gr.	
Nitre		4. 18.
Residuum		2. 15.
Sand, &c.		0. 11.

Loss 1. 14.

Twelve Grains of the Powder gathered and put into separate Boxes, after firing with Ball out of the short Piece, as before-mentioned, being fired in the exhausted Receiver, sunk the Mercurial Gage from Twenty-nine Inches One-tenth to Twenty-three Six-

tenths. And the same Weight of fresh Powder being fired in the same manner, sunk the Gage to Twenty-two Inches Three-fourths; the Difference being $\frac{85}{100}$ or $\frac{17}{20}$ of an Inch.

From these Experiments the Committee are of Opinion, that the First Part of the First Question, *Whether all the Powder of the Charge be fired?* is sufficiently determined in the *Negative*.

As to the Second Part of the First Question, *Whether all the Powder that is fired, be fired before the Bullet is sensibly moved from its Place?* the Committee are of Opinion, *That the Bullet is sensibly moved from its Place, before all the Powder that is fired, has taken Fire.*

This, indeed, has not been determined by any direct Experiment, but seems a Consequence of the Determination of the First Part of the Question, that the Whole of the Charge is not fired.

For let it be considered, that from the Moment any Part of the Powder within the Barrel takes Fire, the Flame of the Powder already fired is always contiguous to some Part of the Powder as yet unfired; and consequently some Part of this last must be continually taking Fire, so long as any unfired Powder remains within the Barrel; that is, the firing of the Powder cannot be over, till all the unfired Powder is driven out of the Gun: But before any Part, how small soever, of the unfired Powder is driven out of the Gun, the Bullet which lies between the Charge and the Muzzle, must necessarily have been driven out of the Gun. Therefore the firing of the Powder is not over, or all the Powder that is fired, is not fired, till after the Bullet is driven out of the Gun. And
con-

consequently the Bullet must be sensibly moved from its Place, before all the Powder that is fired, has taken Fire.

As to the Second Question, *Whether the Distance to which the Bullet is thrown, may not become greater or less, by changing the Form of the Chamber, though the Charge of Powder and all other Circumstances continue unchanged?*

The Committee are of Opinion, *That the Change of the Form in the Chamber, will produce a Change of the Distance to which the Bullet is thrown.* Their Opinion is grounded upon the following Experiments, in which the *longest Chamber* of equal Capacity drove the Ball farthest.

Three brass Chambers were made, whose Depths were respectively Three Inches; one Inch and half; and Three fourths of an Inch; so turned as to fit the Chamber of Mr. *Hauksbee's* Mortar; each of these Chambers contained, when full, One Ounce *Troy* of Powder. The Ball was of Brass, weighing Twenty-four Pound Six Ounces and an half *Avoirdupois*, that is, nearly Three hundred Fifty-six Ounces *Troy* *.

The Ball touched the Powder of the Charge in all these Experiments. With the First Chamber of Three Inches deep, the Elevation of the Mortar being Forty-five Degrees, the Ranges at Four different Trials were found to be,

<i>Shot.</i>	<i>Chains.</i>	<i>Links.</i>	
I. . . .	11.	39. or nearly	752 Feet.
II. . . .	10.	38.	685.
III. . . .	11.	17.	737.
IV. . . .	11.	10.	733.

* Supposing 14 Ounces 11 Penny-weight and 15 Grains and an half *Troy*, equal to 1 Pound *Avoirdupois*.

In the Second of these Experiments, the brass Chamber, not being sufficiently thrust home before the Discharge, was by the Violence of the Powder driven in so, that it could not be got out again without the Help of an iron Screw, and a vast Force applied to iron Wedges. This was doubtless the Cause of the great Irregularity observed in this Case. The mean Distance, collected from the other Three Experiments, is nearly 741 Feet.

Then Three Discharges were made with the Chamber Threc-fourths of an Inch deep, with Ball, Powder and Elevation, as before. The Ranges were,

<i>Shot.</i>	<i>Chains.</i>	<i>Links.</i>	
I.	7.	6.	or 466 Feet nearly.
II.	7.	2:	463.
III.	7.	2.	463.

The mean Distance to which the Ball was thrown in these Three Experiments is 464 Feet.

The Chamber One Inch and an half deep, was also tried; but this not fitting the Mortar so well as the other Two, the Ranges were found to be very irregular, being

<i>Shot.</i>	<i>Chains.</i>	<i>Links.</i>	
I.	10.	40.	or nearly 686 Feet.
II.	9.	6.	598.
III.	7.	8.	467.

The last Shot, falling so much short, may be ascribed to the Damp, it being late in the Evening when it was fired.

That Moisture greatly weakens the Effect of Powder, is commonly known; and the Committee found by an Experiment, That Powder dried by means of a Phial in *Balneo*, and put warm into the Chamber, threw

threw the Ball twice as far as the same Quantity of Powder taken out of the same Barrel, before it was dried.

IV. *An Account of a Meteor seen near Holkam in Norfolk, Aug. 1741. transmitted to the ROYAL SOCIETY by the Right Hon^{ble} Thomas Lord Lovell, F. R. S.*

*Read Nov. 4.
1742.*

T *Thomas Savory, John Walker, and others of Lord Lovell's Ploughmen, being at Plough about the Middle of August 1741. on a fair Day, at Ten o'Clock in the Morning, saw on a Heath about a Quarter of a Mile from them, a Wind like a Whirlwind, come gradually towards them, in a strait Line from East to West. It passed through the Field where they were at Plough, tore up the Stubble in the ploughed Ground, and also the Grass besides the same, for Two Miles in Length, and Thirty Yards in Breadth. When it came to some Closes at the Top of a rising Ground called Ferrybush-Closes, Philip Henning, and others, who were houghing Turneps, saw it appear like a great Flash or Ball of Fire. After having seen the Wind come into the Closes, Robert May was in a Cottage where he lives by a Road-side, at the Bottom of the Park, about a Furlong down-hill from the Close, when one of his Children about Six Years old, who was playing at the Door, cried out, That Ferrybush-Closes were on Fire; on which he went out to look, but saw no Fire, only a terrible Smoak, and heard such a Noise as Fire makes when a Barn is burning.*

burning. He then saw the Wind come from the Cloſes in the ſame manner as before-mentioned, making a terrible Noiſe, like that of a violent Fire, alſo like Carts over ſtony Ground, which paſſed by his Houſe, tearing up the Stones in the Road, and tore up a Rank of Pales, and ſprung ſeveral of the Poſts out of their Places, and carried a pewter Plate that ſtood on the Outſide of the Window, about Forty Yards from the Houſe; and a large Box-cover about an Inch and half thick and Four Feet ſquare, and croſs-barred, which he covers his Birds with, was carried away much further, and torn all to pieces; and the Gravel flew about, and alſo the Flint-ſtones like Feathers. It alſo broke down ſome of Mr. *Knotts's* Fences, and frightened the Cattle in a terrible manner. And, what is moſt remarkable, that every-where elſe but in this Place, the Weather was clear and fine, and no Sign of any Storm or Diſturbance whatſoever. About a Quarter of an Hour after, *Philip Henning*, and Two of his Partners, *Turnep-bougers*, who were at Work about Two Furlongs off, came to the ſaid *Robert May*, and told him, they were glad to ſee him alive; for they expected, that he and his Family, Houſe and all, had been deſtroyed, having ſeen the Fire go that Way, and heard a Noiſe, as if the Houſe had been demolifhed. *Robert May* ſmelled a moſt terrible Smell of Sulphur, both before and after the Wind paſſed him, and heard the Noiſe a great while after ſeeing the Smoak, before he ſaw the Wind, an Hedge intercepting his Sight. He ſays it moved ſo ſlowly forward, as to be near Ten Minutes in coming from the Cloſes to the Houſe.

V. *An Account of the Proportions of the English and French Measures and Weights, from the Standards of the same, kept at the ROYAL SOCIETY.*

Read Nov 11.
1742.

SOME curious Gentlemen both of the ROYAL SOCIETY of *London*; and of the ROYAL ACADEMY of SCIENCES at *Paris*, thinking it might be of good Use, for the better comparing together the Success of Experiments made in *England* and in *France*, proposed some time since, that accurate Standards of the Measures and Weights of both Nations, carefully examined, and made to agree with each other, might be laid up and preserved in the Archives both of the ROYAL SOCIETY here, and of the ROYAL ACADEMY of SCIENCES at *Paris*: Which Proposal having been received with the general Approbation of both those Bodies, they were thereupon pleased to give the necessary Directions for the bringing the same into Effect. In consequence of which, Mr. *George Graham*, Fellow of the ROYAL SOCIETY, did, at their Desire, procure from Mr. *Jonathan Sisson*, Instrument-maker in *Beaufort-Buildings*, Two substantial brass Rods, well planed and squared, and of the Length of about 42 Inches each, together with Two excellent brass Scales of Six Inches each, on both of which one Inch is curiously divided by diagonal Lines, and fine Points, into 500 equal Parts: And upon each of the Rods Mr. *Graham* did, with the greatest Care, lay off the Length of Three *English* Feet, from the Standard of a Yard kept in the
Tower

Tower of London. He also at the same time directed Mr. *Samuel Read*, Scale and Weight-maker near *Aldersgate*, to prepare, in the best manner he could, Two single *Troy* Pound Weights, with Two Piles of the same Weights, decreasing from Eight Ounces to One Quarter of an Ounce respectively, Two Parcels of the lesser corresponding Weights, that is to say, from Five Penny-weight to half a Penny-weight, and Grain Weights from Six Grains to One-fourth of a Grain ; together with Two single *Avoirdupois* Pound Weights : All which, when made, were carefully examined, and found to agree sufficiently with each other. Things being thus provided, the Two brass Rods, one of the Six-inch Scales, and one Set of all the Weights, were sent over to *Paris*, one of the Rods to be returned, and all the other Particulars, to be presented for their Use, to the ROYAL ACADEMY of SCIENCES there : Who, upon Receipt thereof, desired the late *Monsieur Du Fay*, and *Abbé Nollet*, both Members of the ACADEMY, and also Fellows of the ROYAL SOCIETY, to see the Measure of the *Paris* Half-toise, containing Three *Paris* Feet, accurately set off upon both the brass Rods, in the like manner as the Length of the *English* Yard, containing three *English* Feet, had already been set off on the same : After which those Gentlemen returned over one of the Rods to the ROYAL SOCIETY, together with a Standard Weight of Two Marcs, or Sixteen *Paris* Ounces, accompanied with a *Process Verbal*, or Authentic Certificate from the proper Office, of the due Examination thereof.

The Rod being returned, Mr. *Graham* caused Mr. *Sisson* to divide both the Measure of the *English* Yard, and the *Paris* Half-toise, each into Three equal Parts,
for

for the more ready taking off both the *English* and *Paris* Foot from the same: After which, both this Rod and the Two *Marc* Weight sent over from *France*, were, together with the other Particulars before-mentioned, carefully laid up in the Archives of the ROYAL SOCIETY, where they now remain, as their Duplicates do in those of the ROYAL ACADEMY of SCIENCES at *Paris*: But as, before they were so laid up, an accurate Examination and Comparison of them was made by Direction of the Council of the ROYAL SOCIETY, the Result of the same is here subjoined as follows: That is to say,

1. The *Paris* half Toise, as set off on the Standard in the ROYAL SOCIETY, contains *English* Inches by the same Standard 38.355. Whence it appears, that the *English* Yard and Foot is to the *Paris* half Toise and Foot, nearly as 107 to 114. For as 107 to 114, so is 36 to 38.35514.

2. The *Paris* Two *Marc*, or 16 Ounce Weight, weighs *English* Troy Grains 7560. Whence it appears, that the *English* Troy Pound of Twelve Ounces or 5760 Grains, is to the *Paris* Two *Marc* or 16 Ounce Weight, as 16 to 21: That the *Paris* Ounce weighs *English* Troy Grains 472.5, and that consequently the *English* Troy Ounce is to the *Paris* Ounce, as 64 is to 63.

3. The *English* *Avoirdupois* Pound weighs Troy Grains 7004, whence the *Avoirdupois* Ounce, whereof 16 make a Pound, is found equal to 437.75 Troy Grains: And it follows of consequence, that the Troy Pound is to the *Avoirdupois* Pound, as 88 to 107 nearly; for as 88 to 107, so is 5760 to 7003.636; that the Troy Ounce is to the *Avoirdupois* Ounce, as

80 to 73 nearly; for as 80 to 73, so is 480 to 438; and lastly, that the *Avoirdupois* Pound and Ounce is to the *Paris* Two *Marc* Weight and Ounce, as 63 to 68 nearly; for as 63 to 68, so is 7004 to 7559.873.

4. The *Paris* Foot, expressed in Decimals, is equal to 1.0654 of the *English* Foot, or contains 12.785 *English* Inches.

VI. *A Method of making a Gold-colour'd Glazing for Earthen-Ware; communicated, in Latin, in a Letter from M. Godofridus Heinſius, Aſtron. Prof. at St. Petersburgh, to Mr. Peter Collinſon, F. R. S.*

Read Nov. 11.
1742.

TAKE of Litharge parts iij. of Sand or calcined Flint p. i. pound and mix these very well together, then run them into a yellow Glass with a strong Fire. Pound this Glass, and grind it into a subtile Powder, which moisten with a well saturated Solution of Silver, make it into a Paste, which put into a Crucible, and cover it with a Cover. Give at first a gentle Degree of Fire, then increase it, and continue it till you have a Glass, which will be green. Pound this Glass again, and grind it to a fine Powder; moisten this Powder with some Beer, so that by means of an Hair Pencil you may apply it upon the Vessels, [or any Piece of Earthen-ware]. The Vessels that are painted or cover'd over with this Glazing, must be first well heated, then put under a Muffle, and as soon as the Glass runs, you must * smook them, and take out the Vessels.

* *Afflare; debes fumum.*

Numb. 466.

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

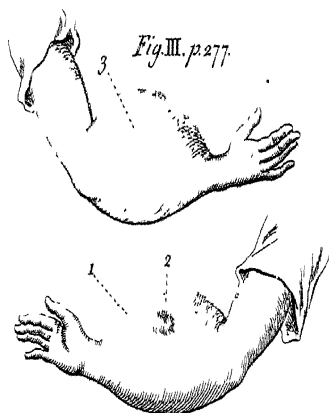
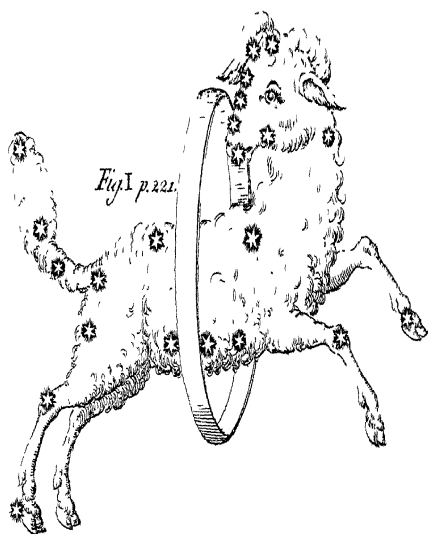
INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

L O N D O N :

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PHILOSOPHICAL TRANSACTION

For Part of *November*, and the Month of *December*,
1742.

The CONTENTS.

- I. *Excerpta ex Ephemeridibus Meteorologicis Romanis Anni 1741. observante Didaco de Revillas, Abbate Hieronymiano, in Romana Academia Matheseos Professore, REGIÆ SOCIETATIS Londinensis, necnon Academiæ Instit. Scient. Bonon. Sodali.* Pag. 193.
- II. *Extract of a Letter from J. F. Gronovius, M. D. at Leyden, November 1742. to Peter Collinson, F. R. S. concerning a Water Insect, which, being cut into several Pieces, becomes so many perfect Animals.* 218.
- III. *Some Conjectures concerning the Position of the Colure in the ancient Sphere; communicated in a Letter from the Rev^d Ebenezer Latham, M. D. and V. D. M. to Dr. Mortimer, Secr. R. S.* 221.
- IV. *The Case of an Extraordinary Dropsy, communicated in a Letter from Tho. Short, M. D. to C. Mortimer, M. D. Sec. R. S.* 223.
- V. *Part*

The CONTENTS.

- V. *Part of a Letter from ——— of Cambridge, to a Friend of the ROYAL SOCIETY, occasioned by what has lately been reported concerning the Insect mentioned in Page 218. of this Transaction.* 227.
- VI. *A Synopsis of the Calculation of the Transit of Mercury over the Disk of the Sun, the 25th of October 1743. by Mr. J. Catlyn.* 235.
- VII. *A Letter from Mr. R. Campbell of Kernan, to Dr. Mortimer, Sec. R. S. concerning a Man who lived Eighteen Years on Water.* 240.
- VIII. *An Account and Abstract by Geo. Hadley, Esq; F. R. S. of the Meteorological Observations communicated to the ROYAL SOCIETY, for the Years 1731, 1732, 1733, 1734 and 1735.* 243.
- IX. *A short Account by James Parsons, M. D. F. R. S. of a Book intituled, Traité des Sens, &c. by M. le Cat, M. D. F. R. S.* 264.
- X. *Observationes variæ Medico-Chirurgicæ a Johanne Daniele Schlichting, Med. & Chir. Doctore, Acad. Cæsareo-Leopoldin. Nat. Curios. Membro, & Commercii Literarii Norimberg. Socio.* 270.
- XI. *Observationes duæ Anatomico-practicæ, una de Infante nato cum Sacco Aqua pleno, ab Ossæ Sacro usque ad Talos propendente; altera de Hydrocephalo singulari. Auctore Job. Baistero, M. D. R. S. S.* 277.
- I. Finis.

- I. *Excerpta ex Ephemeridibus Meteorologicis Romanis Anni 1741. observante Didaco de Revillas, Abbate Hieronymiano, in Romana Academia Matheseos Professore, REGIÆ SOCIETATIS Londinensis, necnon Academiæ Instit. Scient. Bonon. Sodali.*

*Read at a
Meeting of the
Royal Society,
November 18.
1742.*

ANTEQUAM observationes ex nostris Meteorologicis Ephemeridibus depromtas, & per menses singulos distributas proferimus, pauca de instrumentis quibus illas perficiebamus præmittere necessarium ducimus.

Barometro inprimis perfectissimo utebamus, quod Parisina simul ac Londinensi, quam heic adhibebimus, scala in pollices, pollicisque duodecimales lineas divisa, mobilique per eam discurrente indice munitur. Cubiculum in quo persistit, parum supra clivi Capitolini medium situm est. Mediam proinde humiliora inter, & eminentiora urbis loca, regionem servat.

Fahrenheyiano simile est mercuriale quod adhibebamus thermometrum. Inferiorem tamen phialam neque sphæricam, neque cylindricam, neque alterius in thermoscopiis usitatæ figuræ; sed ad scutellæ modum, hemisphæricam concavam habet; ut promptius tota mercurii inter duos hemisphæricos vitri parietes contenti massa, atmosphæræ variationibus obediat; atque interim ab vitri variationibus, quæ calore aut frigore intensiore gignuntur, mercurii adscensus, &

descensus in tubo, nullatenus vitientur. Tota instrumenti capacitas in partes 5000 divisa: quarum priores a summitate tubi numeratas scala in tot gradus distincta dimetitur, quot tubus ipse capere potest. Aquæ ebullientis calore mercurius ad scalæ initium, tubique summitatem conscendit. Frigore vero aquæ in glaciem abeuntis, ad gradus 178 subsidit: ac demum ipsius glaciei frigore, ad gr. 180. Quod si frigus intensius fuerit, descendente infra hosce gradus mercurio, frigoris incrementum majore graduum numero indicabitur. Hocce ergo thermometrum extra fenestram Cæciæ vento, & Capitolii gradibus obversam, locatum, nunquam directo, & non nisi minus reflexo solis radio percutitur. Atmosphæræ proinde calorem frigusve accurate indicare aptum est.

Pluviam Halleiano more cylindrico vase colligebamus, cujus altitudo pedis Londinensis dodrantem, diameter pedes duos cum triente emetitur. Ex eo in aliud cylindricum vas operculo clausum, pedem unum cum altitudine, tum diametro mensurans, aqua per epistomium exeipitur, ne in vapores emittatur. Inde, cessante pluvia extrahitur, & cylindrico similiter vase, pedem unum exactissime alto mensuratur, cujus diameter decimam partem ad amussim æquat diametri majoris vasis, e cœlo delabentem pluviam immediate excipientis. Quoniam ergo horunce vasorum diametri sunt ut 10 & 1; eorum bases erunt ut 100, & 1.

Ob reciprocam itaque æqualium cylindrorum cum basibus altitudinem, aqua in ampliore vase unum pollicem alta, centum altitudinis pollices in minore æquaret. Hoc est, quot aquæ pollices minor hicce cylindrus metitur, tot partes pollicis centesimæ altitudinem pluvix ostendunt. In minore autem vase,
vir-

virgula ad id expresse divisa nedum pollices dimittimur, verum & pollicis decimales partes, quarum proinde singulæ millesimam digitalis altitudinis pluviae partem demonstrant.

Quater in die observationibus plerumque vacabamus: pluries quandoque: Summo nimirum mane, meridie, vespere post solis occasum; & duabus circiter ante mediam noctem horis. Æstivo tempore secunda etiam vel tertia hora post meridiem.

Quas heic afferemus thermometri observationes hyberno tempore frigus indicaturæ, ad matutinas horas referendæ erunt, nisi aliter moneamus. Tunc enim temporis, ceteris paribus, frigidior aër deprehenditur. Æstivo vero tempore ad meridianas, vel pomeridianas horas erunt referendæ, quibus calor intensior.

MDCCXLI.

Mense JANUARIO.

Quamquam postremis duobus superioris anni mensibus tanta aquarum copia e cœlo cecidit, quanta fere prioribus decem; admodum nihilominus pluviosus Januarius quoque erat. Iidem siquidem venti, qui proximis præcedenti solstitio diebus imperium in atmosphæra gesserant, quive Romanum cœlum pluvium plerumque efficiunt, etiam prioribus mensis hujusce diebus dominabantur: Orientales nimirum, australes, & inter hos medii. Frequens proinde pluvia, cœlumque nubibus plerumque obductum. Contigerat plenilunium post mediam noctem primi mensis diei. Barometrum hocce tempore intra octo vel decem lineas supra pollices 29 vagabatur. Thermoscopium majus tunc frigus indicaverat. Gradibus 158, eo longe minus quod prioribus Novembris diebus antecesserat,

cesserat, quibus per vicos & plateas gelaverat; thermoscopio gr. 178 commonstrante.

Die 8 boreales venti, & præsertim ex N. N. E. præpollere cæperant: Cœloque nubibus deterso serenitas amœna redibat; quæ usque ad diem 17 fere semper perseverans, vix aliquando vel matutina nebula, vel dispersis nubeculis paullulum turbabatur. Barometrum tamen, quod die 10 ad lineas $10\frac{1}{2}$ supra poll. 29 conscenderat, deinceps continenter descendebat: eodemque die 17 ad poll. 29. 4. subsidebat flante E. N. E. Noctu nihilominus dierum 16 & 17 flante borea gelabat: Thermometro gr. 180 indicante. Et hoc quidem maximum fuerat hujusce anni frigus. Secunda porro lunæ quadratura contigerat post mediam noctem diei 9; & novilunium post meridiem diei 17.

Die itaque 18 australibus ventis, orientalibus, & euro-notis iterum prævalentibus barometrum ad poll. 29. 1. flante Volturmo descenderat. Et hæc minima fuerat toto hocce anno mercurii altitudo, quamquam die quoque 25 ad poll. 29. $1\frac{1}{4}$ subsidisset. Frequentissimi proinde imbres usque ad diem 28; sed diebus præsertim 25 & 26 insignes hæc barometri depressiones affluentissime subsequiebantur. Die enim 25 duos aquæ pollices; die 26 pollicem cum triente colligebamus. Hisce diebus thermoscopium a gradibus 156 & 160 summo mane plerumque haud recedebat. Prima lunæ quadratura post mediam noctem diei 23 contigerat.

Præter imbres quos diebus 25, & 26 delapsos dicebamus, solutæ etiam australibus ventis montium nives, magnam Tiberi aquarum copiam intulerant; qui proinde die 27 alveum excedens, nedum campos urbi adjacentes, verum & humiliora urbis loca alte inundabat. Colunнарum bases in Panthei porticu aquis
ope-

operiebantur. Hæ porro pedis dodrante altius conscenderant, quam in altera fluminis alluvie diei 7 superioris mensis.

Postremis Januarii diebus boreales iterum venti, & præsertim ex N. N. E. serenitatem advexerant: Barometro ad poll. 30. 0 $\frac{1}{4}$ conscendente. Frigidiores aërem gradus 169 in thermoscopio mane ostendebant. Post solis occasum diei 31 plenilunium celebrabatur.

Frigus maximum hocce mense thermometer indicaverat ad gr. 180.

Maxima barometri altitudo fuerat poll. 30. 1.

Minima————— poll. 29. 1.

Pluviæ altitudo————— poll. 6. 847.

Mense FEBRUARIO.

Vix ad primum & alterum mensis hujusce diem serenitas perseverabat: tertio jam, tenui delabente pluvia, nubes cœlum obduxerant; quæ per vices usque ad diem 8 sæpe comparebant: Barometro ne vix quidem ab altitudine poll. 29. 10. recedente; ventisque vel australibus, vel orientalibus, leniter spirantibus.

Die 8, in quem ante medium noctem ultima lunæ inciderat quadratura, flante primum E. N. E. dein Borea, poll. 29. 11 $\frac{1}{2}$ jam attigerat barometrum; & constanter fudo resplendente, die 14 ad maximum, quam hocce anno conspeximus altitudinem conscenderat poll. 30. 5. At neque serenæ noctes, neque Boreales venti ad diem usque 24 perdurantes, gelu cogere valebant, si diei 20 præcedentem noctem excipias, qua humor in pruinam concretus domestici viridarii herbas contexerat:

rexerat: Thermometro ad gr. $177\frac{1}{2}$ subsidente. Die 15 flante Borea novilunium celebratum fuerat; & prima lunæ quadratura flante N. N. E. die 22 post meridiem.

Barometrum usque ad diem 19 supra poll. 30 linearum 3 vel 4 altitudinem servaverat. Mox pedetentim descendendo, die 25 ad poll. 29. $9\frac{1}{2}$ devenerat. Tunc nubilum apparebat coelum; proximisque diebus 26 & 27 tenuis pluvia, alternatim flantibus Austro & Euro, decidebat. Varia proinde usque ad mensis finem cœli facies, varii ventorum flatus: Barometro vix recedente ab altitudine poll. 29. 10. Frigus mane hisce diebus moderatum: Thermometro inter gr. 165 & 169 cōsistente.

Maximum ergo frigus hocce mense gr. $177\frac{1}{2}$.

Maxima barometri altitudo ————— poll. 30. 5.

Minima ————— poll. 29. $7\frac{3}{4}$.

Pluviæ quantitas ————— poll. 0. 200.

Mense MARTIO.

Februarium varia tempestate desinentem turbido Australium ventorum, nubiumque, apparatu Martius exipiebat. Plenilunium die 2 post meridiem, flante similiter Austro, nubilo cœlo peractum. Australibus deinde ventis orientales & euro-noti permixti, usque ad diem 9 crebros imbres nubisque frequentes adduxerant. Barometrum ab mensis initio quotidie crescens, die 3 ad poll. 29. $3\frac{1}{4}$ descenderat: & quamquam eo die iterum poll. 29. 6³ attigisset, adscensumque continuaret, brevis tamen pluvia sero decidebat. At uberior & pollicem superans post novum baro-

barometri descensum die 6, flante S. S. E. delabebatur : atque iterum, sed minor, flante E. die 9.

Die 10, quo ultima lunæ quadratura post meridiem habebatur ; barometro iterum ascendente, ventoque N. N. E. flante, amœna redibat ferenitas, quæ Borealibus prævalentibus ventis, & die præsertim 13 vehementissime flantibus, (quibus quandoque, sole occidente, occidentales succedebant) ad diem usque 26 perseverabat. Venti porro iidem boreales occidentalibus permixti nedum novilunium diei 17, & primam lunæ quadraturam diei 23 comitabantur ; verum proximos quoque æquinoctio dies. Et tamen ante ipsum debiles, validiores tamen post effecti, prægelidum aërem intempestivo frigore efficiebant ab die 24 ad 27 ; præcipue vero die 26, quo noctu gelabat. Hinc in jam florecentibus arboribus præcocium fructuum germina in universum arefcere conspiciantur. Thermometrum eadem nocte gr. 179. proximis vero grad. 175. ostendebat. Barometrum autem, quod mane diei 22 poll. 30. $1\frac{3}{4}$ attigerat, ipso die ac sequutis descendebat paulatim ; & die 26 poll. 29. $8\frac{1}{2}$ indicabat.

Postridie ergo, flante S. S. E. nubes, & pluvia ; quæ ad postremum usque mensis diem, Volturmo sæpius spirante vento, plerumque perseverabant. Barometrum inter lineas 6 & 9 supra poll. 29 vagabatur. Thermometrum vero summo mane inter gr. 165 & 168 iisdem diebus degebat.

Maximum frigus hoc mense fuerat	gr.	179.	
Maxima barometri altitudo	poll.	30.	2.
Minima	poll.	29.	$2\frac{1}{2}$.
Pluviæ quantitas	poll.	2.	034.

Mense APRILI.

Prior mensis hujusce medietas nullum præter primum, quo plenilunium contigerat, & alterum diem undequaque serenum habebat. Boreales venti, per raro, & non nisi oriente sole spirabant: Australes, orientales, hisque affines frequentissime. Crebri proinde, etsi exigui imbres. Cœlum nubibus persæpe obductum. Diem 9 mane nebulosum, & obscurum, mox pluvium ultima lunæ quadratura nocte præcedente antecesserat. Barometrum plerumque circa poll. 29. 6. vagabatur. Aliquando ad poll. 29. 2. die 10 descendebat; nec nisi die 13 & sequentis constanti adscensu erigebatur. Aëris plerumque moderata temperies. Diebus tamen 7, & 8, Cæcia flante, frigidiuscula; thermometro ad gr. 165 subsidente.

Die 15, quo post meridiem novilunium celebrabatur, ad poll. fere 30. 1. barometrum conscenderat. Venti boreales hisque affines præpollere jam coeperant; atque a die 16 ad 20 aërem nonnihil rigidum advehabant; thermoscopio inter gr. 161 & 163 mane commorante. Serenitas cœli, barometri elevationi conjuncta, ad diem 22 perseverabat; quo post solis occasum prima lunæ habebatur quadratura: & barometrum ad poll. 30. 1 $\frac{3}{4}$ adscendens, eodem, & sequuto die, flante O. S. O. iterum descendebat: Nubibus cœlum obtegebatur.

Die 24 hora Italæ 15 $\frac{1}{4}$ thermometro gr. 158, barometro poll. 30. 0 $\frac{1}{2}$ attingente, cœlo fere ubique sereno, leniterque flante E. N. E. brevis terræ concussio a pluribus percepta, in Etruria finitimisque provinciis validior, Moream præsertim ingentibus afficiebat damnis.

Re-

Reliqui mensis dies admodum varii; ventis nunc australibus, nunc orientalibus, cœlo nunc sereno, nunc nubilo, sed impluvio, spirantibus. Barometrum interea tensim deprimebatur; ac die 30, quo plenilunium ante mediam noctem celebrandum erat, ad poll. 29. $5\frac{3}{4}$ jam descenderat vehementer flante S. S. E. cœloque subnubilo.

Frigus maximum hoc mense fuerat gr. 165.

Maxima barometri altitudo ——— poll. 30. $1\frac{2}{3}$.

Minima ——— poll. 29. 3.

Pluviæ quantitas ——— poll. 2. 035.

Mense M A I O.

Validus auster primum mensis diem tenebricosum efficiebat utut barometro nonnihil ascendente. Declinato jam die, occidentali primum, mox boreali subeunte vento nimbus cum grandine decidebat. Barometrum poll. 29. $6\frac{1}{2}$. Thermoscopium gr. 164 obtinebat. Aer proinde admodum frigidus; præsertim diebus 4, & 6. Sed die 5, flante borea, cœloque sereno, multo rigidior, in vineis suburbanis anniversaria congelatione vites cogebat. Ceterum prioribus mensis diebus cœlum raro serenum; frequentes, sed exigui imbres, frequentissimæ nubes. Eodem die 5, poll. 30 barometrum tenebat; iterumque usque ad diem 11 sensim demittebatur ad poll. 29. $5\frac{3}{4}$; dum interim die 10, hoc est, biduo post ultimam lunæ quadraturam, copiosissimus imber deciderat.

Ab undecimo ad quartumdecimum diem, quo novilunium post sequentem mediam noctem celebrandum erat, vehementissime flantibus nunc cæcia, nunc zephyro, iterum barometrum ad poll. 29. 11.

fereno fere semper cœlo adscendebat. Ventorum deinceps continua vicissitudo, occidentalibus tamen præpollentibus, denuo demittebatur barometrum: dicque 16 brevis decidebat pluvia, postquam restituta serenitas; barometro iterum adscendente. Die tamen 20 post novam ejus depressionem tenuis iterum pluvia austro flante.

Reliquis mensis diebus occidentales venti, australibus plerumque interflantibus, variam cum cœli faciem, tum aëris tempestatem efficiebant; iidemque venti primam lunæ quadraturam die 22, & plenilunium die 30 comitabantur. Barometrum utut perpetuis pariter variationibus obnoxium, non admodum tamen a lineis 9 supra poll. 29 recedebat: die excepto 17, quo fere poll. 30 attingebat. In hac elevatione usque ad postremum mensis diem permanebat. Duobus ultimis diebus cœlum serenum, sed nebulosum.

Exceptis prioribus quinque mensis diebus, reliquis thermoscopium inter gr. 153, & 156 oriente sole versabatur. Circa meridiem tamen etsi plerumque gr. 150 attingebat; nihilominus diebus 16, 19, 11, 13, & 19, increscente calore, austroque flante, gr. 142 indicabat.

Maxima barometri altitudo	} poll.	30.
hocce mense fuerat————		
Minima —————	poll.	29. 4 $\frac{3}{4}$.
Pluviæ quantitas —————	poll.	2. 299.

Mense JUNIO.

Cœlo fere semper serena facie resplendente occidentales venti prioribus quatuor mensis diebus dominabantur.

nabantur. Barometrum, quod primo die poll. 29. 10 $\frac{1}{8}$ attigerat, nonnullis intercurrentibus variationibus pedetentim descendebat; ac die 4 post matutinum lenem eorum flante O.N.O. brevis tempestas, brevique pluvia, cœlum turbabat. Euri deinde sequutis diebus plerumque flabant; & barometro vix itidem descendente, nova & copiosior cum tonitru pluvia, zephyro spirante, decidebat die 6 post meridiem, quo tempore ultima lunæ quadratura celebrabatur. Pluvium quoque sequuto triduo cœlum erat; orientalibus ventis iterum prævalentibus: & barometro intra lineas 8, & 9 supra poll. 29, spatiente. Eoque iterum die 11 nonnihil demisso, brevis denuo pluvia, flante S.S.O. subsequabatur.

Serenitas dein raro nubibus adspersa orientalibus, & occidentalibus ventis per vicem alternantibus; sed his plerumque prævalentibus, ad mensis finem im-pluvie cœlo durabat. Circa meridiem tamen sæpe ex austro lenis flabat aura, quæ, sole occidente, zephyro cedebat.

Barometrum, quod die 12 ad poll. 29. 11. con-scenderat, ibidem toto permanebat mense; quandoque tamen supra poll. 30. elevabatur.

Novilunium ante meridiem diei 13, & prima lunæ quadratura post sequentem mediam noctem diei 21 fereno, placidoque cœlo contigerant. At plenilunium die 28, barometri demissio, & nubes zephyro flante comitabantur.

Dies itaque æstivo solstitio proximi admodum placidi fuerant.

Thermometrum hocce mense plerumque mane inter gr. 146, & 150: meridie inter gr. 138, & 144 versabatur.

Maxima barometri altitudo fuerat poll.	30.	1 $\frac{1}{2}$
Minima ————— poll.	29.	8.
Pluviae quantitas ————— poll.	0.	762.

Mense QUINTILI.

Sereno similiter adspectu decem priores mensis hujusce dies splendebant; tametsi Africi venti hisque affines plerunque dominarentur. Tunc porro temporis barometrum supra poll. 30 constanter permanerat. Sed jam eo sensim descendente, nubes sequutis diebus borealem plagam occupare, sole praefertim oriente, inceperant.

Ultima lunæ quadratura post vesperam diei 5 contigerat; sequentique biduo occiduus horizon plurimum declinato jam sole rubuerat, eodem flante Africo. Novilunium vero die 12 post vesperam similiter celebratum erat; quo tempore coruscationes ad euro-borealem plagam frequentes conspiciebantur. Postridie Africus idem ventus circa meridiem vehementissime furebat: coelo nubibus undequaque obducto. Post occasum solis tranquilla restituta serenitas.

Die 14 ad poll. 29. 8. jam descenderat barometrum; nubesque huc illuc diffractæ apparebant. Sed iterum ad lineas 9 conscendendo, iterumque descendendo, varium coeli comitabatur adspectum, imbresque praeveniebat, qui demum vespera diei 18, tonante, atrisque nubibus obducto coelo, decidebant, Libonoto flante. Crebriores tamen subsequutis tribus diebus delabebantur; & praefertim die 20, dum praecedente nocte barometrum ad lineas 6 $\frac{1}{2}$ descenderat, flante primum euro, mox subsolano.

Post

Post occasum diei 20 prior lunæ quadratura contigerat nubilo, pluvioque, ut prædiximus, cœlo; quo tempore validissimus cæcias-aquilo; aliique boreales venti imperium cæperant, & sequuto etiam die bacchabantur. Mitiores postmodum effecti ad diem usque 25 atmosphæram tenebant. Barometrum jam denuo eodem die 21 fursum vespere ferebatur; ejusque adscensu continuante, amœna serenitas ad postremos fere mensis dies, nubeculis dumtaxat aliquando depicta perseverabat.

Diebus interim 27, & 28, euro primum, dein Austro, & Africo leniter spirantibus, barometrum nonnihil deprimebatur. Sole vero, diebus 28, 29, & 30, occidente, nebulosa ac densa Zona, horizontem occiduum, cœlo ceteroquin sereno, obtenebrabat. Die autem 31, nebula in nubes condensata, iterumque descendente barometro, copiosissimus imber, euro flante, demittebatur.

Thermometrum die 7 duabus circiter post meridiem horis gr. 128 attingebat; & die 17, hora eadem, gr. 122: qui quidem caloris gradus nequàm mensis hujusce, verum & currentis anni maximus fuit, quamquam sextili etiam mense ad eundem gradum, ut mox videbimus, calor pervenerit. Reliquis diebus mane inter gr. 140, & 143. post meridiem inter gr. 132, & 135 plerumque versabatur.

Maxima barometri altitudo fuit	} poll.	30.	0 $\frac{3}{4}$.
erat hoc mense			
Minima	poll.	29.	6 $\frac{1}{2}$.
Pluviæ quantitas	poll.	3.	629.

Mense SEXTILI.

Primo etiam mensis hujusce die, etsi barometrum constanter adscenderet; & mane cæcias-aquilo spiraret,

raret, succedente tamen zephyro, tenuissima decidebat pluvia; postquam vento eodem perseverante, & coro per vicem flante diu serenitas perseverabat; quæ diebus dumtaxat 5, 6, & 7 densa nebula matutinis horis offundebatur: Barometro tunc temporis prope poll. 29. 11. commorante; & affinibus boreæ ventis per vices flantibus. Die 9 poll. 30. Barometrum superabat; at eo iterum ad mox dictam altitudinem demisso, iterum, sed densior nebula diebus 12, 13, & 14 coelum mane obtenebrabat Libonoto constanter flante.

Ultima lunæ quadratura post mediam, quæ diem 4 antecesserat, noctem: & novilunium mane diei 11, Africo eodem spirante vento, celebratum fuerat.

Libonotus, qui, ut prædiximus, die 14 flabat, usque ad diem 18 cum zephyro alternans plerunque dominabatur. Die autem 15 in thermoscopio gradus $122\frac{1}{2}$; die 16 gr. 122 secunda post meridiem hora majorem hujusce mensis indicabant calorem, qui etiam totius anni maximus fuerat, ut superiore mense notabamus. Mitius dein flantibus austro, & zephyro, calor quoque remittebatur; ac barometro nonnihil decedente coelum nubibus obtegebatur. At ingravescente nocte, ventis iterum permutatis, barometrum denuo elevabatur; redibatque serenitas. Sequenti die luna primum quadrantem post meridiem attingebat.

Serenitas, ventis boreæ affinibus spirantibus, ad diem 23 durabat. Sed Libonoto illis iterum succedente, nubes, & tonitrua subsequiebantur. Dein noctu pluvia, barometro poll. 29. 8 $\frac{1}{4}$ monstrante decidebat. Mane flante euro restituta serenitas triduo perseverans, barometro parum variante. Borealibus interim prævalentibus

valentibus ventis; & præsertim die 27 post brevem nocturnam pluviam tonitribus comitatam, validissime flantibus, rigeat aër: Thermometro ad gr. 149 mane subsidente: reliquis vero sequutis diebus ad mensis usque finem, inter gr. 148 $\frac{1}{2}$, & 147, percurrente.

Intempestivo huic aëris rigori tribuebant vinitores, quod in suburbanis vinetis, uvarum jam maturescen-
tium obdurata cutis, acinique exsiccati, gracilem ad-
modum vindemiam tulerint, quam de cetero race-
morum copia affluentem spondebat.

Barometrum interea, cæcia plerumque flante, poll.
29. 10 $\frac{1}{2}$ attigerat. Sed postremo mensis die, eo non-
nihil depresso, nubes circa meridiem cælum obducebant.

Maxima ejus altitudo fuerat	} poll.	30.	0 $\frac{3}{4}$.
hoc mense —————			

Minima —————	poll.	29.	8 $\frac{1}{4}$.
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Pluviæ quantitas —————	poll.	0.	486.
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Mense S E P T E M B R I.

Prioribus quinque mensis hujusce diebus varia
cœli facies, varia tempestas; sudo nubiloque alter-
nantibus. Mane pluries cœlo sereno in boreali plaga
nubeculæ apparebant. Cæcias oriente sole plerumque
spirabat: Auster circa meridiem. Barometrum inter
lineas 8, & 10 supra poll. 29 vagabatur. Et quam-
quam vespera diei 5, lineas 11 attigisset; iterum
tamen noctu nonnihil descendebat; & sequenti die,
Africo flante, pluvia demittebatur. Hoc ipsum obser-
vabamus die 8; quo post pomeridianam pluviam, ac
tonitrua, borealibus euro mixtis ventis prævalentibus,
serenitas redibat ad diem 12 perseverans.

Mane

Mane diei 2 ultimum quadrantem Libonoto flante, luna compleverat. Die vero 9 post solis occalum conjunctionem celebraverat, spirante coro.

Dies 12 fere totus serenus, cæcia primum, deinde zephyro flantibus. Barometrum sensim deprimebatur. Nocte ingravescente, cœlum subnubilum. Flabat eurus. Postridie, eodem spirante vento, & barometro ad poll. 29. 9 lente demisso, ingens imber duos fere altitudine pollices æquans decidebat. Nubes tamen sequuta nocte cæcias eliminabat; & barometrum per lineæ semissim elevabatur. Serenitas deinceps usque ad finem mensis nubibus quandoque turbata perstabat. Frequenti etiam nebula cœlum obfuscabatur, diebus præsertim 23, 24, 25, & 30, sole oriente. Primum quadrantem spirante austro, cœloque sereno ante solis ortum diei 17 lunæ pertransierat; & oppositionem ante ortum diei 25 eodem flante vento absolverat.

Post diem 20 barometrum motu vario parum ab altitudine poll. 30 recedebat. Mane plerunque venti boreales orientalibus permixti, sero occidentales permixti australibus spirabant. Hicç autem atmospheræ status proximos autumnali æquinoctio dies comitabatur: poll. 30 barometro itidem servante. Sub mensis finem flabat N. N. E.

Thermoscopium mensis hujusce periodo admodum varios frigoris calorisque gradus cum mane, tum post meridiem ostendebat. Nam prioribus tribus diebus sole oriente inter gr. 140, & 142: Meridie inter gr. 130, & 133 versabatur. Deinceps plurimum variabat. Pluries enim oriente sole ostendebat gr. 152, quandoque 153 $\frac{1}{2}$. Post meridiem, plerunque inter gr. 134, & 138 divagando, circa mensis finem gr. 126 $\frac{1}{2}$
at-

attingebat, qui maximus totius mensis calor fuerat Zephyro tunc spirante.

Maxima barometri altitudo fu-	} poll. 30.	$1\frac{1}{4}$.
erat hoc mense —————		
Minima —————	poll. 29.	$7\frac{1}{2}$.
Pluviæ quantitas —————	poll. 2.	605.

Mense OCTOBRI.

Rerum novitate notabilis admodum mensis hicce erat. Ultimum quadrantem post solis occasum diei 1, spirante Cæcia, cœloque sereno, luna pertransierat. Qui sub mensis superioris finem præpollebat ventus N.N.E. prioribus quoque mensis istius diebus perseverabat. Mox occidentales ad diem usque 12 perflantes succedebant. Barometrum paullo vel infra, vel supra poll. 30 consistebat. Nocte vero quæ diem 8 sequebatur poll. 30. $4\frac{1}{4}$ attingebat.

Nocte eadem sereno cœlo hora post occasum solis IV, Aurora Borealis adparebat, totam, boream inter, & occasum, occupans cœli plagam insigni rubedine splendescentem. Urbis ædificia partem cœli horizonti proximam inspicere prohibebant. Post horæ intervallum color rubicundus in candicantem sensim permutabatur: paullo post lumen extinguebatur; iterumque occasum versus, debilius tamen, redibat. Brevi penitus deficiebat. Zephyrus tunc lenissime flabat; & thermoscopium gr. 148 metiebatur.

Lumen hocce boreale in Placentino etiam agro, sed adspæctu diverso observatum, nobilissimi & Cl. V. *Ubertini* Marchionis *Landi* nos epistola docuit. Ibi post primam ab occasu solis horam adparebat. A Borea nonnihil occasum versus primum declinans ter-

tiam cœli partem albicante illustrabat colore. Tenuissimæ nubes stantium columnarum instar, fixorum lumen haud intercipientes ex horizonte adfurgebant; vixque sensibili mutatione variabant. Nocte integra perleverabat albicans splendor. At aurora imminente rubrum induebat colorem, orientem versus aliquantulum declinando. Ad ortum itaque, naturali nascentis auroræ luce cœlum albescebat: ad septentriones insueto jam veluti adolescentis auroræ rubeſcebat lumine: atque hoc pacto aurora duplici eodem tempore illustrabatur. Sequuta etiam diei 9 nocte iterum, sed dilutiore luce usque ad quartam ab occasu solis horam in eodem Placentino agro conspiciebatur. Evanescente dein lumine obscurior præter consuetudinem, & tenebricosior nox succedere videbatur. Romæ, cœlo quamvis sereno, & novilunium eadem nocte luna celebrante, nullum insuetæ lucis vestigium observabamus.

Ab die 9 sensim descendebat barometrum, permanente sudo, & zephyro continuante. Die tamen 12 ventus hicce circa meridiem densissimis nubibus cœlum antea serenum obtenebrabat. Mox barometro ad poll. 29. $11\frac{3}{4}$ delapso ingens adeo imber demittebatur, ut pollices fere duos, & semisi altitudine æquaret.

Paullo post solis occasum redibat serenitas; & sequenti mane, Euro flante nubes ad borealem dumtaxat horizontem conspiciebantur. Barometrum vespere sudo adscendebat ad poll. 30. $0\frac{3}{4}$. etsi Africus primum, dein Auster spiraret.

Mane diei 14 iterum Africus, mox Auster circa meridiem flabant; & post meridiem pluebat. Sequento etiam biduo, nunc Austro, nunc Cæcia flantibus
pluvia

pluvia decidebat. Die autem 16, qui primum lunæ quadraturam antebat, tonitrua, & horrificæ coruscationes pluviam densissimam comitabantur. Sed declinante die jam serenitas restituta. Barometrum hisce diebus immotum fere poll. 29. 10 $\frac{1}{2}$ tenebat usque ad diem 19, quo, occidente sole, paullulum descenderat. Sequuta autem nocte imbres delabi incipiebant, qui, barometro supra poll. 29. 9 plerunque commorante, fere ad mensis usque finem perseverabant. Copiosissimi tamen diebus 21, 23 & 24. Et, quod notatu dignissimum, die 23, validissimo flante Austro, tonitru, grandine & fulminibus comitantibus, quinque fere aquæ pollices, sex circiter horarum intervallo decidebant. Barometrum eo die ad poll. 29. 6 $\frac{1}{2}$ paullo ante descenderat; & thermometer gr. 149 indicabat. Die etiam 24 ad pollicis altitudinem pluebat; tametsi barometrum nonnihil iterum adscendisset. Sequenti nocte plenilunium. Reliqui dies, exiguo continuante barometri adscensu variam cœli faciem, sed plerunque nubilam ostendebant; borealibus ut plurimum spirantibus ventis Euro permixtis.

Thermometrum toto ferme mense inter gr. 151 & 154 mane versabatur. Meridie inter gr. 139 & 144. Sed postremis mensis diebus mane inter gr. 156 & 160. Meridie inter 151 & 152.

Maxima barometri altitudo fuerat	} poll. 30. 4 $\frac{1}{2}$.
hoc mense —————	
Minima —————	poll. 29. 6 $\frac{1}{2}$.
Pluviæ quantitas —————	poll. 10. 895.

Mense NOVEMBERI.

Cæcias, eique ad latus flantes venti, toto ferme hoc mense, vario cœlo spirabant. Barometrum poll. 29. 10 constanter superabat, diebus 7 & 8 exceptis, quibus flante Volturmo ad poll. 29. 8 descenderat: largusque imber die præsertim 8 decidebat, quo post meridiem novilunium peractum erat. At iterum pristinam elevationem recuperante, & continuo superante barometro, die 16, quo primum quadrantem lunæ pertransierat, poll. 30. 4 $\frac{1}{2}$ attingebat flante Borea. Hæc porro barometri altitudo ab maxima hujusce anni altitudine parum deficiebat.

Oriente sole diebus 17 & 18 thermometer gr. 170 & 171 sereno cœlo, & Borea flante metiebatur. Deinceps diebus 20 & 21 cito descensu, concidente ad poll. 29. 8 $\frac{1}{2}$ barometro, pluvia iterum, tonante, & coruscante cœlo, Austro autem flante decidebat. Thermometrum inter gr. 155 & 161 tunc temporis mane versabatur. Sequuto biduo, dum scilicet post occasum diei 22 pleno orbe splendebat luna, barometro ad poll. 30. 2. elevato, serenitas redierat; & thermometer ad gr. 166 mane subsidebat. Sed barometro nonnihil iterum delapso iterum pluvia. Novo dein interjecto serenitatis biduo (quo thermometer ad gr. 174 $\frac{1}{2}$ rigidior aër, & spirans aquilo mane deprefferat) flantibus denuo Cæcia, & affinis ventis, usque ad mensis finem vel nubilum, vel pluvia. Die porro 28 barometri descensus insignior poll. 29. 1 $\frac{3}{4}$ attingebat. Brevi tamen eodem regnante vento, iterum passim elevabatur: dum interim continuata, sed tenuis decidebat pluvia: ultimumque quadrantem nocte diem 29 sequuta, egerat luna.

Hocce

Hocce itaque mense ab insigni altitudine, fere ad minimam, dierum duodecim intervallo barometrum descenderat.

Maxima ejus altitudo fuerat ——— poll. 30. $4\frac{5}{8}$.

Minima ——— ——— ——— poll. 29. $1\frac{3}{4}$.

Pluviæ quantitas ——— ——— ——— poll. 3. 488.

Mense DECEMBRI.

Quos superiore mense dominatum in atmosphæra habuisse dicebamus affines Cæciæ venti, hocce etiam mense usque ad diem 20 præpollere observabantur. Cælum plerunque serenum erat, sex prioribus exceptis diebus, quibus adscensu, descensuque vario barometrum intra poll. 30 & 29. 9. divagabatur: descensum pluvia etsi tenui subsequente. Reliquis decem & quatuor, aut supra, aut parum infra poll. 30 consistebat. Mane autem diei 8 (quo tempore plenilunium abolvebatur) & sequentis diebus 10 & 11, ad poll. 30. 2. conscenderat. Post diem 15, quo prima lunæ quadratura peracta post meridiem fuerat, nonnihil jam ex altitudine poll. 30. $1\frac{1}{2}$ demittebatur, nubilumque adparebat cælum.

Thermometrum, quod paullo ante solis ortum diei 2, gr. 174 attigerat, sequentis usque ad diem 11 diebus, eadem hora intra gr. 165 & 170 versabatur. At frigore crudescente, ad gr. 174 $\frac{1}{2}$ die 13 consistebat. Deinceps frigus mitigabatur.

Circa meridiem diei 20 Australes venti spirare, obtenebrari cælum, ac barometrum deprimi observabantur. Descensus hicce nocte sequenti præcipitantiior, tres & amplius lineas metiebatur; poll. 29. $5\frac{1}{4}$ mane indicando. Mox ergo ante meridiem diei 21 ronante cælo, ingenti Austri flatu, largus imber effundebatur.

Die

Die 22 ante meridiem iterum, Africo spirante, pluebat; oppositionem obrinente luna. Et quoniam solstitium hybernium die 21 occidente sole contigerat, venti proximis eidem solstitio diebus imperantes, Australes plerumque fuerant, Africo, Volturmo, Euroque permixti; cœlum deinceps ad mensis usque finem, eisdem Volturmo & Euro flantibus, pluvium constanter erat: solaque diei 26 periodo plusquam tres & semiss pluviae pollices decidebant: Barometro poll. 29. 10 $\frac{1}{2}$ commonstrante. Dum vero die 28 ad poll. fere 30 pervenerat, alter pluviae pollex sequuta nocte demittebatur; iterumque nocte altera pluebat, dum ultimum quadrantem luna superabat. Quo tempore hanc imbrum copiam cœlum effundebat, thermometer gr. 161 plerumque aëre admodum miti, mane commonstrabat.

Maxima barometri altitudo fu-	}	poll.	30.	2.
erat hoc mense				
Minima		poll.	29.	3 $\frac{1}{2}$.
Pluviae quantitas		poll.	8.	501.

OBSERVATIONES GENERALES in Ephemerides Meteorologicas Anni 1741.

Quandoquidem Februario mense maximam barometri altitudinem poll. 30. 5; Januario autem minimam poll. 29. 1 metiebantur; scala variationum mercurii in barometro fuerat hocce anno linearum 16, seu pollicis cum triente; altitudo vero media poll. 29. 9.

Eodem Januario mense frigus totius anni maximum in thermometro ostendebant gr. 180; mensibus vero Quintili & Sextili maximum calorem gr.

122. Scala ergo variationum mercurii in thermometro fuerat graduum 58, seu totidem earum partium, qualium tota thermometri capacitas 5000 complectitur. Quapropter demtis ex posteriore hocce numero partibus 122, quas maximo regnante calore mercurius haud occupabat; istius volumen, eo temporis, partes dumtaxat 4878 obtinebat. Harunce autem partium, maximo adveniente frigore, amittebat 58. Seu si mercurii volumen calore maximo ampliatum in partes 1000 divisum ponamus maximo accedente frigore, partes ab eo millesimæ $11\frac{8}{9}$ amittebantur. Eadem enim præter propter, inter 4878 & 58 ratio interest, quæ inter 1000 & $11\frac{8}{9}$.

Quantitas pluvix hoc anno delapsæ erat poll. 43. 780. Ex superiorum autem octo annorum observationibus, media pluvix quantitas poll. 34 adæquabat. Anno autem 1737, qui reliquos imbrium copia superabat, pollices dumtaxat 36. 788 decidebant. Anni itaque 1741 pluvia, mediam excedebat poll. 9. 780; & maximam anni 1737 poll. 6. 992. Quod porro minime prætereundum est; tribus tantummodo postremis anni mensibus pluvix pollices 22. 884 decidebant, qui nedum dimidiam totius hujusce anni pluvix quantitatem, verum duas tertias mediæ annuæ pluvix partes superabant. Major ergo imbrium copia hocce anno tribus postremis mensibus demittebatur.

Quintilis etiam mensis imbres, quos poll. 3. 629. mensurabant, insueti admodum fuerant; quum, ab eo saltem, quo hisce observationibus vacamus, tempore nunquam eodem mense integrum aquæ pollicem decidisse deprehenderimus. Atque id fortasse in causa fuerit, cur æstivi fructus vermibus plurimum, hocce anno scatuerint: quemadmodum nimiis etiam

autumnalibus imbribus, vermium, quibus oleæ corruptæ fuerant, grassantem copiam agricolæ tribuebant.

Aliud quoque insolens phænomenon autumnales pluvias comitabatur. Pluries quandoquidem barometro supra mediam altitudinem haud parum elevato, copiosissimi diurnique imbres decidebant. Quamquam inficiari non possumus, fere semper ante imbrium delapsum ex maiore altitudine mercurium aliquantulum descendisse.

Quemadmodum aliis annis, ita & præsentī, rarissime Cori venti, frequentissime Euri sub Romano cœlo flare observabantur. Euri porro, & Australes aut nubilum, aut pluvium plerunque cœlum efficiebant: sudum Boreales, & Zephyri. Aliquando tamen, etsi raro, hi pluviam, illi serenitatem advehabant.

Qui vero proximis utrique solstitio diebus crebrius perflabant, sequuta tempestate diei frequenterque dominabantur. Quod in ventis etiam prope æquinoctia spirantibus, utut non adeo sensibiliter, notabamus. Hanc porro observationem longa annorum serie Romæ abs se comprobata V. Cl. *Franciscus Blanchinius* affirmabat; nosque plerunque constantem deprehendebamus.

Tranquillo præterea cœlo, æstivo præsertim tempore, venti orientales matutinis horis, australes circa meridiem, & sole occidente occidentales lenissime spirare plerunque observabantur.

Acus magnetica declinabat hocce anno a Borea in occasum gr. 15. 40. Incrementum propterea declinationis ab anno 1730, erat gr. 4. 40. Eo siquidem anno declinatio fuerat gr. 11.

Pauca

Pauca nunc de morbis popularibus hujusce anni subjicimus, quæ ab illustrissimo *Leproto*, Archiatro Pontificio, & Regiæ Societatis sodali.

Ad hyemem, paucis pectoris inflammationes contigerunt; Catarrhi vero, tonsillarum inflammationes, & rheumatici affectus, pluribus. Cæperant quoque circa Januarii finem malignæ febres petechiales in pluribus grassari, iis præsertim qui prope Tiberim habitabant: quibus plures simul in eadem domo corripiebantur. Et continuæ quidem omnino febres illæ erant, nihilque intermittebant; sed omnes earum more invadebant, quæ ad tertianarum naturam prorsus accederent; uno quidem die leviores, altero vehementiores. Has inter cetera symptomata dolor capitis vehemens comitabatur; cui sequutis diebus coma vigil, & diarrhœa superveniebant. Curabantur autem feliciter sanguinis missione, diluentium, & chinæ-chinæ usu, omissis omnino cum emeticis, tum purgantibus, quibus uti apud aliquos in more positum est.

Circa veris finem præfatas febres aliæ prorsus intermittentes excipiebant, quæ quidem, ut plurimum, boni moris erant; cephalalgia tamen gravi fere semper conjunctæ: & hæ pariter Peruviani corticis usu, & phlebotomia profligabantur. Atque hoc febrium genus non modo per æstatem, & autumnum, verum etiam præsentem in hyemem grassantur.

Ad æstatem pariter diarrhœas & choleras passi sunt nonnulli. Vulnere putredinem, seu corruptionem contrahebant; unde febres ad depurgationem usque. Tandem quibusdam ad hanc præsentem hyemem spuria pectoris contigerunt inflammationes, & catarrhi. Plerique tamen ægroti prædictis febribus intermittentibus laborant.

Duobus postremis anni mensibus complures repente occubuerunt, ex apoplexia alii, alii vero ex internis præcordiorum affectionibus. Illud denique animadversione dignum censemus, frequentes per æstatem, & autumnum fuisse morbos ex vermibus ortos, quos in acutis etiam vomitu, & dejectionibus expulsos vidimus: idque in rusticis potissimum observare proclive erat.

II. *Extract of a Letter from J. F. Gronovius, M. D. at Leyden, November 1742. to Peter Collinson, F. R. S. concerning a Water Insect, which, being cut into several Pieces, becomes so many perfect Animals.*

S I R,

Read Nov. 18.
1742.

IT is now about Nine Months since that a young Gentleman, living in the Family of *Mynheer Bentinck* at the *Hague*, discovered a Water Insect, not known yet or described by any Author. It has a pellucid Body, having here and there branched out something like Claws, with which it catches a particular sort of small Worms, which are every-where found in standing Waters: These are its Food.

But of what Sort this Insect is, is not known; nor have its Mouth, Stomach or Intestines been yet discovered.

But what is most surprising is, that, cut this Animal in Five or Six Pieces, in a few Hours there will be as many like their Parent.

This

This Discovery was and is very surprizing to all our Virtuoso's, and really not believed, until the Professors *Albinus* and *Mussenbrock* were provided with the Animals, and, after having well examined this Creature, found the Prodigy of increasing itself in that wonderful Manner, very true.

One of the Gentlemen that made this Discovery was Mr. *Allemand*, a Man of great Learning and Ingenuity, Tutor to the Sons of Mr. *s'Gravenfande*.

There

The first Account given to the ROYAL SOCIETY, of this surprizing Property of the fore-mentioned Insect, was in a Letter from *Monsieur Buffon*, of the *Royal Academy of Sciences at Paris*, and F. R. S. to *Martin Folkes*, Esq; now our President; his Letter bears Date the 18th of *July 1741*. N. S. and was communicated to the SOCIETY at their next Meeting on the 29th of *October* following; and therein *Monsieur Buffon* acquaints Mr. *Folkes* of Two very singular Observations lately made in Natural History; the First of a small sort of Bug, which produces its Like somewhat after the Manner of Plants, and without Copulation; the other of a small Insect called a *Polypus*, which is found sticking to the common *Duck-weed*, and which, being cut in two, puts out from the upper Part a Tail, and from the lower a Head, so as to become Two Animals instead of One; besides which, when cut in Threë, the middlemost Part puts out from one End a Head, and from the other a Tail, so as to become Three Animals, all living like the first, and performing the Offices of their Specie. Both which Observations Mr. *Buffon* says were well averred.

Mr. *Folkes* alio at the same time communicated another Letter he had received from the Honble *Charles Bentinck*, Esq, at the *Hague*, dated the 15th of the foregoing *September*, wherein it is said, That a young Gentleman of *Geneva*, then in *Holland*, whose Name we since learn to be *Monsieur du Tremblay*, had found in Water, wherein he was looking for Insects, some small things he at first took for Plants, till, on a further Examination, he perceived them to move, and to contract themselves on their being touched; nor could he at first think them to be Animals, by reason of several young Shoots he found to come out from them, and to hang upon one another as far as the fourth Generation: He was, however, at last satisfied they were Insects, and that they preyed upon others, and would even eat raw Flesh. They fixed themselves,

There have been several of these wonderful Creatures sent to *Paris*, to Mr. *Reaumur*, from whom we hope for a particular Disertation.

But after all, I do not think it a perfect Animal, but a kind of the *Uvæ marinæ*, *Holothuria* or *Zoophyta*, which really are living when they are first caught; of this Kind are the *Pennæ marinæ*, figured by *Barrelierus* in Table 1273 and 1774. and also the *Fungi Marini*, Tab. 1293, 1294. These last I remember I have found several times on our Sea-Coasts, and observed that there was a living Nature in them.

he said, by one End to some Plant, or the Side of the Vessel in which they were contained, and at the other End had Six or Eight Arms, with which they seized their Prey. He also found, that one of them being cut asunder, a few Days after, new Arms were grown out of that Part that had none before; since which he had cut them every Way possible, in Length, Breadth, and obliquely, and always with the same Success; after which he has gone on still further, subdividing them, but never found them to propagate any other ways than by Shoots, several at a time, and without any Copulation. Mr. *Bentinck* added, That this Gentleman would soon print an Account of the Observations he had made; and that the Insects he had himself seen of this Sort, were from about a Line to half an Inch in Length.

The late Mr. *Lewenhoeck* seems to have met with this same Sort of *Animalcula* in the Year 1703, and has described and given a Draught of them in a Letter published in N^o 283. of these *Transactions*. Soon after which a more perfect Draught and Description of the same Insects were inserted from an anonymous Hand in N^o 288. of the same *Transactions*; all which Figures answer very well to the Description and a rough Sketch in Mr. *Bentinck's* Letter. In Fig. III. and IV. of this last cited *Transaction*, one of the Insects is represented as quite pursed up or contracted; but neither Mr. *Lewenhoeck*, nor the last-mentioned anonymous Author, ever thought of dividing the Insect, though the latter had observed the young Shoot dropping off from the Parent.

III. *Some Conjectures concerning the Position of the Colure in the ancient Sphere; communicated in a Letter from the Rev^d Ebenezer Latham, M. D. and V. D. M. to Dr. Mortimer, Secr. R. S.*

S I R,

Read Nov. 18.
1742.

I Have an Opportunity of transmitting to you a Draught of the Constellation *Aries*, as it was exactly copied by Dr. *White*, from a Book in the fine Library of your learned Uncle *Samuel Sanders*, Esq; a worthy Member of the ROYAL SOCIETY. I do not know whether it may not be esteemed of some Moment towards the determining the famous Controversy with respect to Sir *Isaac Newton's* Chronology. Dr. *Halley* observes*,
 “ That the Dispute is chiefly, Over what Part of the
 “ Back of *Aries* the Colure passed. Sir *Isaac Newton*
 “ takes it to be over the Middle of the Constellation;
 “ P. *Souciot* will have it, that it passed over the Mid-
 “ dle of the *Dodecatemorion* of *Aries*, which by
 “ Consequence would make it pass about Mid-way
 “ between the Rump and First of the Tail;” which
 Situation could never be said to be over the Back: Whereas, if the Ring in this Cut [see in the TAB. Fig. 1.] was designed, as I apprehend, to image the

* *Philosophical Transactions*, N^o 397.

Colure in the *ancient Sphere*, it exactly answers *Hipparchus's* Description—ἐν δὲ τῷ ἐτέρῳ κολέρω φησὶ καὶ οὕτως τῆς κριῆς τὰ νῶτα κατὰ πλάττω. and justifies the Construction *Sir Isaac* put on those Words beyond Exception. The Sculptures from whence this was taken, have the Title of *Aratæa*; *sive Signa Cælestia, in quibus Astronomicæ Speculationes Veterum ad Archetypa vetustissimi Aratæorum Cæsaris Germanici Codicis* (44) *ob oculos ponuntur a Jacobo de Geyn ex Biblioth. Acad. Lugd. Bat. Amstel. 1652.* *

As I have no Opportunity to consult the Original, it might give more Weight to the Account, if the *Connoisseurs* of your learned SOCIETY could support the *Engraver's Testimony* as to the great Antiquity of that. I will only beg Leave to observe farther, that as this Catalogue begins with the *Draco*, which the Ancients seem to place at the Head of their Constellations; perhaps it may give some Light into the Time of the Book of *Job*, as well as into the Sense of that Place. For when he says, Chap. xxvi. 13. *By his Spirit he has garnished the Heavens; his Hand has formed the crooked Serpent*; I submit it to the Judgment of the Critics, whether it is not highly probable the Writer must have lived within that Period of Time wherein a Star of that Constellation might pass for the Polar Star: And then, if the Asterisms are supposed to be placed in some such Order as here, the express Mention that he only makes

* *Hug. Grotii Batarvi Syntagma Arateorum: ex offic. Plantin. 4to.* See *Germanicus's* Interpretation, p. 35. the Figure of the Constellation *Aries*.

of this, was sufficient to refer us to the whole System or Furniture of the Heavens. But I am afraid I become tedious. I am

*Your Illustrious SOCIETY'S,
and your most obedient,
humble Servant,*

Findern, July 8. 1742.

E. Latham.

IV. *The Case of an Extraordinary Dropsy, communicated in a Letter from Tho. Short, M. D. to C. Mortimer, M. D. Sec. R. S.*

Sheffield, Aug. 2. 1742.

S I R,

Read Nov. 18.
1742.

THE following Case being somewhat uncommon, perhaps it may not be wholly unworthy of your Notice. Last *January* I was called to visit a young Woman of Thirty Years of Age, who about Seven Years ago, had like a severe Fit of the Stone in her Left Kidney, with all the common Symptoms of a Stone, but by some Means recovered again. Three Years ago she had another Fit, but got better in a few Days; though she mostly complained of a dull Pain in that Place ever after. When I saw her, I found her *Menses* had been very irregular and small since her last *Paroxysm*, and totally obstructed since *September*; her Pulse very small and quick, her Countenance pale and languid; a Pain at the Pit of her Stomach, towards the

the Spleen, besides that in the Kidney; her whole Stomach and Belly full, and somewhat swelled, but harder on the Left Side than the Right; a Fluctuation of Water or Matter among the *Abdominal* Muscles, and the *Peritoneum* very hard under it: The Right Side was full, and softer. She had no Appetite, little Sleep, a small Cough, a little Thirst, slight Fever, and much Pain. I ordered her some laxative, aperient, attenuating, diuretic Pills, with an antihydropic stomachic Mixture, the Country Air, and daily moderate Riding. She pursued this Method a few Weeks with some Advantage, but not so much as she expected and desired. ~~Then she took the Advice~~ of another Physician, to no better Purpose. In *April* I was consulted again by her. Her Flesh was now much shrunk, the Belly fuller, Pulse quicker, Pain the same, Urine scanty, but pale, Appetite languid. I prescribed other things to the same Purpose as above, but with no better Success. In *June* I put her on drinking *Newil-Holt* Water (which last Year had cured Three of Dropsies, which was all had used it for that Purpose) and riding: Upon this she made Water freely, slept tolerably well, had a better Appetite, less Pain, and much chearfuller; but the Swelling of the Belly was still the same. Always on turning her in Bed, she heard and felt a Jolting and Fluctuating of Water in the Belly: This put her on being tapped, not doubting but she would recover then. Next Day I was sent for to see the Water drawn off, *July* 23. but to my Surprise, on the Puncture, between Three and Four Pints of very thick, ropy, mixt Matter came away; some was Matter, some a thick white Slime, but the greatest Part was a
thick

thick reddish-brown Liquor, like Liver mashed with a little Water: It could not get through the Canula, without often clearing it with a Goose-quill. After this came near Six Pints of clear Water or Serum, as in a Dropsy. She seemed much easier then, all the Afternoon and Night; next Forenoon not so easy, though she came down Stairs to Dinner. Quickly after it, she was most severely and violently seized with such excruciating Pain all down the Left Side to the Foot, as threw her into the most profuse Sweat, often Faintings, Vomiting, &c. At Four o'Clock, she wholly lost both Sense and Motion of that Thigh and Leg; at Five she was insensible, and at Six she died, *July 24th.*

Next Day the Body was opened before me, when a monstrous Tumour on the Left Side of the Belly shewed itself, and a large Bag of Water on the Right Side appeared, which Two filled the whole Cavity. The *Abdominal* Muscles on the Left Side were very large, flabby, bloated, and a livid pale. The *Peritoneum* uncommonly hard, thick and scirrous; the Liver and Spleen both much emaciated; the first not above Two Pound, the last about Two or Three Ounces; the Stomach and Intestines, from the *Cardia* to the *Anus*, full of small, hard, white, scirrhus Knots, like small Peas, or Hailstones; the Intestines of a dusky yellow Colour: The Remains of the *Omentum* were mortified: The Kidneys were sound: The *Pancreas* very small and yellow. The Tumour on the Left Side (which was the *Ovarium*) being cut up, some Pints of the same Matter as was first drawn off in Tapping, run out: It was divided into innumerable Cells, full of different Matter, some as above; some white, thick Slime,

some fatty, some purulent, &c. The Partitions between the Cells very strong, cartilaginous in the Middle, so as to resist the Knife; like muscular Flesh below and above this Cartilage, so was each Cell. The whole *Ovarium*, before it was first broke, might weigh about Twenty Pound Weight. The Bag of Water on the Right Side, was the other *Ovarium*, wherein was nothing but like a large Ox Bladder, containing Nine or Ten Pints of Water; like a Bladder at the lower End, and rising up like a crooked Horn at the other End; the Skin was very thin and smooth. The *Vesica urinaria* and *Uterus* were both found.

~~From the above-mentioned Account you will see,~~
 1st, That here was a triple Dropsy, viz. One inter-muscular on the Left Side of the *Abdomen*; One in the Cavity of the Belly; One, and the largest of all, in the Right *Ovarium*. 2^{dly}, As I have before observed the like in some others, in much the same Condition; in barren Women, and stale Maids, Tapping should be very cautiously undertaken: Especially when the whole Belly is not equally distended, and not a free Fluctuation of the Water heard and observed from Side to Side, as the Sick turn in Bed; but especially if there was, or is, a sensible Difference to be felt in the Hardness or Softness of the Parts of the Belly, before it is distended monstrously.

I am,

S I R,

Your most humble Servant,

Tho: Short.

V. *Part of a Letter from ——— of Cambridge, to a Friend of the ROYAL SOCIETY, occasioned by what has lately been reported concerning the Insect mentioned in Page 218 of this Transaction.*

Nov. 20. 1742.

Read Nov. 25.
1742.

THE last News from *Paris* gives something very surprising in the Account of *Monsieur Reaumur's* late Memoir, read in the *Royal Academy of Sciences* there, concerning an Animal called a *Polypus*, in which Life is said to be preserved, after it has been cut into several Pieces, so that One Animal seems by Section to be immediately divided into Two or Three more complete Animals, each separately enjoying Life, and continuing to perform the proper Offices of its Species.

Such an Account would have been less regarded, had we not been informed before, that Two * Letters had been communicated to the ROYAL SOCIETY, some Months since, from good Hands, both which mentioned the same thing, and related it as a Fact averred, and carefully examined, by one of the greatest Judges, and most indefatigable Promoters, of Natural History, and especially of that Part of it, which leads to the Knowledge of what is most particular and remarkable in the Insect and Reptile Part of the Creation.

Some of our Friends, who are firmly attached to the general Metaphysical Notions we have formerly learned, reason strongly against the very Possibility of such a Fact: but as I have myself for-

* See above p. 219 and 220.

merly owned to you, on other Occasions, my Distrust of the Truth, or Certainty at least, of some of those Principles, which I never yet had a sufficient Understanding of, to give a full and clear Assent to; I shall now make no Scruple of acknowledging, that I have already seen so many strange things in Nature, that I am become very diffident of all general Assertions, and very cautious in affirming, what may, or may not possibly be.

The most common Operations both of the Animal and Vegetable World are all in themselves astonishing; and nothing but daily Experience, and constant Observation, makes us see, without Amazement, an Animal bring forth another of the same Kind; or a Tree blossom, and bear Leaves and Fruit.

The same Observation, and daily Experience, make it also familiar to us, that besides the first Way of propagating Vegetables from their respective Fruit and Seed, they are also propagated from Cuttings; and every one knows, that a Twig of a *Willow* particularly, cut off and only stuck into the Ground, does presently take Root and grow, and becomes as much a real and perfect Tree, as the original one from which it was first taken.

Here is then, in the Vegetable Kingdom, the very thing quite common, that *Monsieur Reaumur's* Memoir is said to give a rare Example of in the Animal. The best Philosophers have long observed very strong Analogies between these two Classes of Beings: and the Moderns, as they have penetrated further into Nature, have every Day found Reason to extend that Analogy: some have even with great Probability

bility talked of a Scale of Nature, in which she, by an insensible Transition, passed from the most perfect of Animals, not only to the most imperfect, and thence to the most imperfect of Vegetables, but even through Coralline Bodies, and Minerals, to the very Earths and Stones, which seem the most inanimate Parts of our Globe.

Now in such a Scale, who is the Man that will be bold to say, Just here Animal Life intirely ends, and here Vegetable Life begins? Or, Just thus far, and no further, one sort of Operations goes, and just here another sort quite different takes its Place? Or, again, Who will venture to say, Life in every Animal is a Thing absolutely different from that which we dignify by the same Name in every Vegetable? And might not a Man even be excused, if he should modestly doubt, whether Plants and Vegetables may not themselves be considered as a very low and imperfect Tribe of Animals; as Animals might, in like manner, be considered as a more perfect and exalted kind of Vegetables?

We see the Two Sexes of Male and Female run through all the higher Parts of the Animal Creation; yet would he have gone a great deal too far, who should have thence asserted, there were no Exceptions to this general Oeconomy; or that this was one of the general and distinguishing Affections of all the Animal Kind: For modern Discoveries have informed us, that there is somewhat very analogous to this in the Vegetable Creation also: And even in the Animal it has been found, that *Snails*, *Earth-worms*, and some others, are really Hermaphrodites, having in themselves the Organs of both Sexes; whilst the work-
ing

ing *Bee* is truly of no Sex at all, nor any-ways employed in the Production of that Species, it labours so hard daily to provide with Food.

But, whereas, in Animals, the Division of the Sexes is almost general; and the Union of them in one Subject appears but in a few Instances; contrarywise, in Vegetables, almost all have the whole Apparatus of Generation in each Individual, whilst only a few Sorts seem to emulate Animals in what is analogous to the Division of them.

I seem, perhaps, to wander too much from the Point first-mentioned; but as I am only offering loose Hints, and such wild Conjectures as come in my Way, hope to be excused, though I yet hazard another Observation, which is, That what appears chiefly to be new, in the Subject of this Memoir, is, that the Animal or Animals live and do well after their Separation, and that they are capable of re-producing such Parts as the Head and the Tail, which seemed essentially wanting.

I say, that the Animal's living and doing well again, is what is chiefly new; for that an Animal, after Separation of some of the principal Parts, seems for some time to retain Life in each Part, must have been observed by every body*; and though People generally

* The Antients have taken Notice of this, and some even seem to have had no doubt, that Life continued some time in the Parts of a divided Insect. *Aristotle* observes in the Fourth Book of his History of Animals, that almost all Insects live some time when pulled asunder, *Wasps* particularly; and that those live longest, when so separated, which have a long Body, and many Feet; so that the *Scolopendra* being cut asunder, one Part moves on forwards, and the other backwards. But a
Pas-

rally say, from their Prejudice in favour of some of the Principles above hinted at, that to be sure only one of the Parts, though they know not always which, *feels* and has the Sensation of Pain; yet have all I have ever talked with on the Subject, as freely acknowledged, that the *Phænomena* appeared on the other Side.

A *Chicken*, or a *Pigeon*, whose Head is suddenly struck off, shews in both Parts, if no preconceived Opinion led us to think otherwise, strong Signs of Pain and Suffering, and the very same Signs, that the respective Parts of the Animal shew of that Sensation, whilst it is surely living and intire: And I have been told by some, who have seen the Heads of Malefactors suddenly severed from their Bodies, that the same Observation holds also in our own Species. But we have all seen it hold much stronger in the more im-

Passage of St. *Austin* is so remarkable on this Head, that I cannot help transcribing it: *Me revocat quod his hausi oculis Cum enim nuper in agro essemus Liguria, nostri illi Adolescentes, qui tunc mecum erant studiorum suorum gratia, animadverterunt humi jacentes in opaco loco reptantem bestiolam multipedem, longum dico quemdam vermiculum: vulgo notus est; hoc tamen quod dicam nunquam in eo expertus eram; verso namque stylo, quem forte habebat, unus illorum animal medium percussit: tum ambæ partes corporis ab illo vulnere in contraria discesserunt, tanta pædum celeritate, ac nihilo imbecilliore visu, quam si duo hujusmodi animantia forent. Quo miraculo exterriti, caussæque curiosi, ad nos, ubi simul ego & Alypius considebamus, alacriter vivencia illa frustra detulerunt. Neque nos parum commoti, excurrere in tabula quaquaversum poterant, cernebamus; atque rursus ipsorum, stylo tactum, contorquebat se ad doloris locum, nihil sentienti alio, ac suos alibi motus peragentes. Quid plura? Tentavimus quatenus id valeret; atque vermiculum, imo jam vermiculos, in multas partes dividimus: ita omnes movebantur, ut nisi a nobis illud factum esset, & comparerent vulnera recentia, totidem illos separatim natos, ac sibi prorsusquam vixisse crederemus.*

Aug. Lib. de Quantitate Animæ.

perfect

perfect Animals, as they are commonly called, such as *Worms*, where, on the Separation of the Body into two Parts, Life has continued seemingly in both, and with strong Signs of it, longer than we have had the Patience to attend and examine. We have been, indeed, quite uncertain, in which of the Parts this seeming Life has been most conspicuous: and as both Parts have seemed to endeavour to get away, and have frequently soon after been found missing, Boys and ordinary People are generally possessed of an Opinion, that they unite and grow together again after their Separation.

Now, if it could once be allowed, that Animal Life and Sensation might subsist but an Instant, in both Parts of the Creature, after its Section; the whole remaining Difficulty would be only as to the Cure of the Wounds, and the Reproduction of the necessary Organs that are wanting. And, for the first of these, we know very well, that the more imperfect Animals are killed with much greater Difficulty than the more perfect, their Vitals being more diffused, and their general Organization being, I suppose, far more simple than that of the higher Tribes: And as to the other, I think no one will see any Impossibility in the Reproduction of certain Parts, after what we have seen and read of, in the *Lobster* and *Cray-fish* Kinds, who when they chance by any Misfortune to lose a Claw, reproduce it in a short time, with all its Joints, and the proper Muscles for moving them; all which appears as difficult as the regaining of a Mouth and a Tail to some of the *Worm-kind*; whose general Organization being simple, and consisting chiefly of only one strait Gut, or Passage, from the Mouth to the
Vent,

Vent, they seem to want little more to reproduce either, than a Contraction of the Wound, with the Assistance of the Muscles that move the several Rings of which the Body is composed; and every one of which, in its first and natural State, performs almost the same Motions as are necessary for Suction or Ejection: the latter of which we have even sometimes seen very wonderfully supplied in our own Species, in those Cases, where grievous Wounds of the Intestines have put Nature upon trying to perform her Operations in a new Way.

Upon the Whole, we are all very desirous to see *Monsieur Reaumur's* Memoir on this curious Subject; we hope it will soon be published, when, as his curious and exact Experiments will afford infinite Entertainment, so his judicious Remarks upon them will doubtless be no less instructive: but will, in all Probability, give a Light into these Matters we do not yet think of. In the mean time I could not help just mentioning to you, what came into my own Head on the Occasion, hoping that however you may look on my Thoughts as the Dreams, perhaps, of a Man bewildered in his Inquiries into Nature, you will still believe me to be a firm and constant Lover of Truth, and ready at all times to receive and embrace whatever is really such, however odd and surprising it may at the first chance to appear.

I shall therefore only add one or two Facts I should indeed have mentioned before, when I was speaking of the Difficulty of killing some of the Tribes of Insects and Reptiles; which are, that I have myself seen the Heart of a *Viper* continue its regular Beats more than Six Hours after it had been taken out of

the Body: That I have seen that Body move and seem alive to all Purposes for a great Part of the same time, after having lost the Heart; and that I have seen *Wasps*, whose Heads had been taken off, creeping in the Window the next Day; and *Butterflies* that have lived, and attempted even to fly, several Days after undergoing the same severe Operation.

Insects seem at first to suffer but little from the Loss of their hinder Parts, although these contain most of their *Viscera*; nor does the Loss of Limbs seem to affect them in any Proportion to the more perfect Animals. But even in our own Kind, in Infancy, before the Parts have lost all their Softness, much greater Wounds may be received without Loss of Life, than afterwards. If we go yet further back to our *Embryo* State, it is very probable, that yet vastly greater Hurts are recoverable: And it is upon that Principle chiefly, that the best and most likely Account has been given by modern Writers in Anatomy, of some very remarkable Monsters that have appeared in the World, where even some of the most essential Parts of Two *Fetus's* have been seen wonderfully united in One and the same Body. I shall now detain you no longer, than to assure you, that I am, with Truth and Respect,

S I R,

Your most obedient,

Humble Servant, &c.

VI. *A Synopsis of the Calculation of the Transit of Mercury over the Disk of the Sun, the 25th of October 1743. by Mr. John Carlyn.*

Read Nov. 25. 1742.	THE Equal Time of the true \odot at <i>Greenwich</i> . . . }	D H "	
		Oct. 24.	22 15 58
	The Equation of Natural Days add		16 11
	Apparent Time of the true \odot	Oct. 24.	22 32 9
<hr/>			
At which time the true Place of the <i>Sun</i> and of <i>Mercury</i> seen from the Earth }	m,	12 36 44	
The Geocentric Latitude of <i>Mer-</i> <i>cury</i> }	South.	9 37	
The Elongation in $\frac{1}{2}$ Hours (<i>i.e.</i>) the $2\frac{1}{2}$ immediately preceding and following the \odot . . . }		29 16	
The Difference of Latitude in the same time }		4 24	
Therefore the Angle of the appa- rent Way of \odot with the Ecliptic }		8 33 00	
And the Distance of their Centres at the time of their nearest Ap- proach }		9 31	
And the Motion of Interval be- tween that and the \odot . . . }		1 26	
And the hourly Motion of <i>Mer-</i> <i>cury</i> in his Path over the Disk of the <i>Sun</i> }		5 55 $\frac{1}{12}$	
	H h 2		And

And the Motion of the $\frac{1}{2}$ Duration from the first to the last exterior Contacts of the Limbs	13 15
And the Motion of the same for the interior Contacts . . .	13 4
Hence, the Time of the Interval from the δ to the Middle . . .	14 32
of $\frac{1}{2}$ the exterior <i>Transit</i> . . .	2 14 22
of $\frac{1}{2}$ the interior <i>Transit</i> . . .	2 12 30

Hence,

The first exterior Contact of the Limbs	h ' "	} <i>Oct.</i> 25. Morning
The first interior Contact	8 32 19	
The nearest Approach of the Centres, or Middle	8 34 11	
The last interior Contact	10 46 41	} Afternoon.
The last exterior Contact, or End of the <i>Transit</i>	0 59 11	
	1 01 3	

This Computation is made from Tables * which give the ascending Node of *Mercury* at the Time of this *Transit* 6' 17" too forward, according to the Result of very accurate Observations made of that in the Year 1723, by Dr. *Halley*, Dr. *Bradley*, and Mr. *Graham*. Therefore making the Calculation with this Correction of the Place of the Node, the Times of the several Circumstances of the *Transit* will be as follows:

* *Philosophical Transactions*, N^o 386.

	h	'	"	
The first exterior Contact .	8	29	21	} Oct. 25. in the Morn- ing.
The first interior Contact .	8	31	05	
The nearest Approach of the Centres	} 10 46 6			
The last interior Contact .	1	01	7	} Afternoon.
The last exterior Contact .	1	02	51	

This *Transit* may be very aptly compared with that which happened on the 24th Day of *October* 1697 *; as happening at the End of a remarkable Period in *Mercury's* Motion, by which he is nearly in the same Situation, with respect to the *Sun*, at every Completion of it. Dr. *Halley* in his Series of Moments, in which *Mercury* is joined to the *Sun*, &c. (published in the *Philosophical Transactions*, No 193.) makes the Middle of this *Transit* at 11' past Six in the Morning the 24th Day, or the 23d Day at 18^h 11' *p.m.* and the Distance of the Centres of the *Sun* and *Mercury* 10' 04".

It may not be amiss to examine and compare these Numbers by such Observations as were made of this *Transit*, and may be depended on, and thereby to collect the Difference between Computation and Observation; and whatever Error arises in Excess or Defect by a proper Application to the *Transit* of 1743. it is imagined, will foretel it with a greater Degree of Exactness, than a Calculus from any Theory whatsoever.

There was only the Egress of *Mercury* in the *Transit* of 1697. capable of being observed in *Europe* †;

* Mean Period 46 Years 1^d 5^h 43' 42".
 † *Flamsteed's Hist. Cælest. Lib. II. Fol. 32.*

which was done at *Nuremberg* in *Germany*, by Mr. *Wurtzelbaur*, and at *Paris* by *Monsieur Cassini*: At *Greenwich* Clouds prevented it. At *Nuremberg* Mr. *Wurtzelbaur* observed *Mercury* to go off of the Disk of the *Sun* * at $8^h 45^{\frac{1}{2}}$ *mane* about $73^{\frac{1}{2}}$ Degrees from the Vertex of the *Sun* to the Right Hand; and *Monsieur Cassini* observed the same accurately at $8^h 10' 24''$ *mane*; therefore from the known Difference of Meridians of these Places, the Egress must have happened at *Greenwich* at $8^h 1'$ *mane*.

The Observation of Mr. *Wurtzelbaur* will greatly avail at coming at the Duration of the *Transit*. It is mentioned, that *Mercury* left the Limb of the *Sun* $73^{\circ} 30'$ from his Vertex to the Right. Now at that time at *Nuremberg*, the Angle of the Ecliptic with the Vertical passing through the *Sun's* Centre, was $42^{\circ} 3' 5''$; therefore the last Point of Contact on the *Sun's* Limb was observed $31^{\circ} 26' 55''$ from the Ecliptic to the South, and consequently his Latitude was $8' 28''$ South at that time.

To find the Point on the *Sun's* Limb of the Ingress, in order to come at the Duration of the *Transit*, we must be beholden to Computation, and the Theory of *Mercury's* Motion: I have therefore, from the Tables from which the above Times of the *Transit* of 1743 are drawn, carefully computed his Motion along his Path crossing the Disk of the *Sun*, and find that he moved along it after the Rate of $5' 53'' \frac{1}{4}$ in an Hour, and the Difference of Latitude in 5 Hours $4' 21''$, and his Elongation $29' 7''$: Therefore the Angle of his

* Vertex to the Right, it says, *a Nadir Solis ad dextras*; but it is a manifest Mistake, as any one upon Trial may find.

visible Way was $8^{\circ} 29' 50''$, which, doubled, and added to $31^{\circ} 26' 55''$, gives $48^{\circ} 26' 35''$, his Distance, on the Limb of the *Sun* from the Ecliptic also to the Southward at his Ingress on it; therefore the nearest Approach of his Centre to that of the *Sun* was $10' 19''$, and the Length of the Path run during the *Transit* $25' 14''$, and consequently the time of running it $4^h 17'$, the half of which $2^h 8' \frac{1}{2}$, subtracted from $20^h 1'$, the End of the *Transit* at *Greenwich*, gives the Middle there at $17^h 52' 30''$, earlier by $18' \frac{1}{2}$ than the Series of Moments, &c. give it.

Now as the said Series makes the Middle of the *Transit* of 1743, at $11^h 2'$ *mane*, and as it corresponds with that of 1697; and the Computation of that is $18' \frac{1}{2}$ too late by the Series of Moments, &c. it may be reasonably expected, that the same Computation for this of 1743 will be so much too late too; and if so, the Middle may be put down at $43' \frac{1}{2}$ past 10, or $44'$ at farthest, *October* 25th in the Forenoon.

By Computation from the Tables above-mentioned, with the Correction of the Node, I make the Distance of the Centres at the nearest Approach in 1697, to be $10' 33''$, but by the Observations of Mr. *Wurtzelbaur* it turns out only $10' 19''$, less by $14''$. Should therefore their Distance in 1743 computed in the same manner at $9' 10''$ be as much diminished, the Duration of the *Transit* will be protracted no less than $5' 24''$, and the first Contact will be $2' 42''$ earlier, and the last so much later, than the Times above-mentioned for them.

N. B. In the Computation of the *Transit* of 1743, the Semidiameter of the *Sun* is supposed $16' 14'' \frac{1}{2}$, and that of *Mercury* $4'' \frac{1}{2}$; but in that of 1697, have taken

taken *Mercury's* only $3''\frac{1}{2}$, imagining the precise Moments of the first and last exterior Contacts are not observable; but that the Ingress is seen some little time later, and the Egress sooner, than the true times thereof. I have all along spoke of the Motion of *Mercury*, without mentioning that of the *Sun*, whereas, in Reality, it is that of them both jointly; but as we may suppose the *Sun* to stand still during the *Transit*, it will then be considered as the apparent Motion of *Mercury* alone for that Time.

VII. *A Letter from Mr. Robert Campbell of Kernan, to Dr. Mortimer, Secr. R. S. concerning a Man who lived Eighteen Years on Water.*

S I R, *Dec. 1. 1742.*

Read Dec. 9. 1742. THOUGH unknown, at the Request of Mr. *Malcom*, I trouble you with an Account of the extraordinary Abstinence of *John Ferguison*, a Native of the Paroch of *Killmellfoord* in the Shire of *Argyle*.

About 18 Years ago he happened to overheat himself on the Mountains, in Pursuit of Cattle, and in that Condition drank excessively of cold Water from a Rivulet, near by which he fell asleep; he awaked about 24 Hours after in a high Fever: During the *Paroxysm* of the Fever, and ever since that time, his Stomach loaths, and can retain, no kind of Aliment, except Water, or clarified Whey, which last he uses but seldom, there being no such thing to be had by
Persons

Persons of his Condition in that Country for many Months in the Year.

Archibald Campbell of *Ineverliver*, to whom this Man's Father is Tenant, carried him to his own House, and locked him up in a Chamber for 20 Days, and supplied him himself with fresh Water, to no greater Quantity in a Day, than an ordinary Man would use for common Drink; and at the same time took particular Care, that it should not be possible for his Guest to supply himself with any other kind of Food without his Knowledge; yet after that Space of Time, he found no Alteration in his Vigour or Visage.

He is now about 36 Years of Age, middle Stature, a fair and fresh Complexion, with a healthy (though not seemingly robust) fresh Complexion; his Habit of Body is meagre, but in no remarkable Degree; his ordinary Employ is looking after Cattle, by which means he needs must travel Four or Five Miles a Day in that mountainous Country.

He uses no *Tobacco*; yet seems to discharge as much *Saliva* as others, who do not use *Stimulus's* to provoke that Evacuation.

If we may judge of his insensible Perspiration by the Softness and Freshness of Skin, he is in that respect like other Men, and like them sweats with violent Exercise; as to the grosser Excrements, it did not occur to me to inquire about them, but I conclude he discharges none; because the Country People, who strongly fancy him supported by supernatural Means, would not forget to object this to him, if he evacuated any Quantity of gross *Fæces*, with which Water is not charged.

This History of this abstemious Person I had from Mr. *Campbell* of *Ineverliver*, my Neighbour in that Country, who is a Gentleman of great Candour and Ingenuity, neither credulous himself, nor any ways inclined to impose upon the Credulity of others. I had the same Account from several others, and confirmed by the Belief of the whole Country. The Man himself I never saw, but the Bearer, Mr. *Charles Campbell*, Preacher, has conversed with him, on whose Veracity you may depend.

The Case appears extraordinary singular, and worth the Notice of Men of Letters; is *one* Instance to convince us, that a great Part of the gross Meats which we greedily destroy, is not necessary for the Support of Animal Life; and that there must be some other Qualities in the pure Element of Water, than what have fallen under common Observation, since they have supported this Man in Health and Vigour for so many Years, and supplied the Evacuations necessary in the Animal Oeconomy.

S I R,

Your most humble and
most obedient Servant,

Robert Campbell,
of Kernan.

VIII. *An Account and Abstract of the Meteorological Observations communicated to the ROYAL SOCIETY, for the Years 1731, 1732, 1733, 1734 and 1735. By Geo. Hadley, Esq; F. R. S.*

Read Dec. 9.
1742.

THE Diaries that continue throughout the said Five Years, are only those kept at *Crane Court, Southwick, and Coventry*. The *Kentish* Diary for the Year 1731 is wanting, and ends with the Year 1734. I have, in my former Account of the Years 1729 and 1730, given an Account of the Method and Contents of the Two First. Mr. *Henry Beighton's*, from *Griff* near *Coventry*, contains the Height of the Barometer at several times of the Day, in Inches and Decimals, and the Weather. That from *Upsal* by Mr. *Celsius*, from *Hudicksväl* by Mr. *Broman*, and from *Abo* by Mr. *Sporing*, go no farther than the Year 1731; for which Year there is also one from *Lunden* by an Author whose Name I do not find; for it appears not to be Mr. *Conrad Quensel's*, whose end in the Year 1730, from the same Place: It contains Observations on the Barometer twice a Day, in *Swedish* Measure, which I have reduced to *English*; the Wind and Thermometer, which is a particular one of his own.

Mr. *Weidler's* Diary from *Witemberg* continues to the End of the Year 1734. In the Year 1732, he alters his Method of the Barometrical Heights, from *Paris* to *London* Measure, and the Days of the Month from the *New Style* to the *Old* one, to make them

the better correspond with our Observations. He gives a very accurate Account of the *Phænomena* of several Northern Lights in the Ends of the Years 1731 and 1733, and Beginning of the Year 1734. His Diaries also contain some few Astronomical Observations, and extraordinary Occurrences.

Captain *Christopher Middleton's* Journal of his Voyage to *Hudson's-Bay* is published already in *Philosophical Transactions*, N^o 418. The *Naples* Diary by Dr. *Cyrrillus* ends in the Year 1732, and also that from *New-England* by Mr. *Dudley*.

For the Year 1734, that from Dr. *Pack*, at *Canterbury*, exhibits in one View, by a Table for every Month of the Year, in the First Column, the Quantity of Rain, and the Evaporation: In the Second, Third and Fourth, the greatest and least and middle Heights of the Barometer, Thermometer, and Hygrometer: In the Fifth, the Meteors, by Variety of Marks, which he gives an Explanation of: In the Sixth, the Direction and Strength of the Winds. He gives also a Description of the Instruments he invented, and made use of, for Observation of the Quantity of Rain and Evaporation, and the Hygrometer, with a Draught of each. For the Month of *January*, there is a particular Table, containing great Variety of Observations for every Day of that Month. The Thermometer is peculiar to himself, as far as I know; and he gives no Rule to reduce it to the Standard. There is a Letter of his, relating to a Chart of the Levels of *Kent*, which, he thinks, are so contrived as to cause a Circulation of Air from the Sea, which is of great Use. Mr. *Forth's* Diary, at large, from *Darlington*, begins in the Year 1737; but he has given an Abstract for the Three
pre-

preceding Years: In which the greatest, least and middle Height of the Barometer is given for every Month; which Mean, upon Examination, I take to be found in the way used in these Tables, and therefore I have put them in as such. By a Letter of his it appears, his Thermometer stands at Forty-five Degrees, when Mr. *Hawksby's* stands at Thirty-three, which is Twelve Difference; and, I suppose, he means they differ so much throughout the Scale; so by that Rule I have reduced his Observations to the Table. *Quære*, At what Time of the Day the Observations were made, and where the Thermometer was placed; for the mean Heights differ but little from those at *London*, as he observes in his Letter. There is an Extract of a Letter from *Signor Didacus de Revillas* to Dr. *Mortimer*, containing an Account of the Rain that fell at *Rome*, beginning with *August* 1734, and ending with *July* 1735, in *Paris* Measure, which I have reduced to *English*.

Marquis *Poleni's* Diaries, at large, from *Padua*, end in the Year 1730; but he sent an Abstract of his Observations for the Six following Years, which was published in the *Philosophical Transactions*, N^o 448. in which the Account of the Depth of Rain being intire, I have inserted it in the Table, for the readier comparing it with other Places.

These are all the Manuscript Observations communicated to the ROYAL SOCIETY, relating to *Meteorological Observations*. I have added the Observations of the Barometer, Thermometer, and Rain, at *Edinburgh*, from the Four Volumes of Medical Essays; and Mr. *Doppelmaier's* Barometrical Observations,

tions, from the printed ones at *Norimberg*, to make the Tables as general as I could. The Tables are drawn up in the same manner as those for the Years 1729 and 1730; and from them various Observations and Comparisons may be made, in the same manner as has been done by several Hands heretofore, particularly, Dr. *Plot*, Dr. *Derham*, Mr. *Locke*, Marquis *Poleni*, and others, as appears in the *Transactions* of this SOCIETY; and many more such Observations may be added, by those that are curious in these things, at their Pleasure.

A Table of the monthly mean Heights, and also of the greatest Ascents and Descents of the Barometer, observed in several Places, and communicated to the ROYAL SOCIETY, for the Year 1731, in Inches and Decimals.

1731.	Crane-Court, London. Lat. 51° 31'	Southwick in Northamptonsh. Lat. 52° 31'	Coventry in Warwickshire. Lat. 52° 34'	Upsal in Swe- den. Lat. 59° 48'	Huducksal in Sweden. Lat. 60°
January	29 83	29 61	29 44	29 71	29 11
February	78	57	31	98	74
March	30 20	92	72	71	99
April	29 71	51	31	79	86
May	30 00	72	52	79	70
June	29 92	66	48	66	71
July	95	72	51	68	76
August	86	65	48	85	92
September	96	70	49	65	59
October	93	67	47	84	82
November	77	54	33	61	56
December	88	61	39	66	81
Mean of the whole Year	29 89	29 658	29 45	29 74	29 577
† Highest	30 50 Mar. 7	30 11 May 30.	29 56 May 31	30 18 Oct. 29	30 52 Dec. 23
Lowest	29 00 Feb. 12	28 82 Feb. 12	28 59 Feb. 12	28 66 Dec. 6	28 59 Dec. 6.
Differ	1 5	1 29	1 37	1 82	1 53

The

The foregoing Barometrical Table continued.

1731.	Lunden in Swe- den. Lat. 59° 48	Abo in Find- land. Lat. 60 40	Wittenberg in Saxony. Lat. 52°	Naples in Italy. Lat. 41.	Edinburgh in Scotland. Lat. 56 20
January	29 37	29 54	29 48	29 09	
February	43	52	60	11	
March	57	45	70	30	
April	43	55	48	03	
May	65	58	86	14	
June	51	48	73	29	29 05
July	53	48	77	01	07
August	48	66	71	03	07
September	64	47	78	17	06
October	59	28 87	82	06	03
November	34	29 21	47	14	08
December	29	46	77	32	05
Mean of the whole Year.	29 48	29 44	29 66	29 14	29 58
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q Highest	30,91 Jan. 10.	31,12 Dec. 21.	30,21 June 1.	29 4 Sape.	
Lowest	29 1 Jan. 29.	28,90 Dec. 7.	28,66 Jan. 29.	28,76 Jan. 29.	
Differ.	1,81	2,22	1,55	00,94	

A Table of the monthly mean Heights, and also of the greatest Ascents and Descents of the Thermometer, observed in several Places in the Year 1731, and communicated to the Royal Society.

1731.	Crane-Court.	Southwick.	Upsal.	Abo.	Wittenberg.	Naples.	Edin- burgh.
January	70 3 ⁸	72	76 3 ⁸	93 3 ³	82 4	54 5	—
February	64 4	66 5	71 1	98 5	79 9	45 9	—
March	57 2	57 5	66 7	91 5	72 2	45 4	—
April	56 2	57	60 1	82	62 9	38 3	—
May	39 9	42	43 2	68	16	26 6	—
June	36 1	38	38 3	50 5	43 7	17 7	48
July	32 1	36	37 4	50	41 3	12 7	41
August	33 1	37	40 3	49	39 7	14 3	45
September	38 4	42	47 2	61 5	48 9	18 9	49
October	46 6	49	56 3	73	60 7	26 3	57
November	59 9	59 5	65 7	83	66 4	36 9	69
December	63 1	63 5	72 9	90	77 9	49 9	74
Mean of the whole Year.	49 3 ⁸	52 0	56 3 ³	74 3 ¹	60 3 ¹	32 3 ³	—
Thermom Highest	8 July 31.	10 June 31.	16 Aug. 2.	20 June 21.	26 June 27.	5 June 20.	28 July 4.
Lowest	82 Jan. 31.	89 Jan. 8.	91, 6 Jan. 31.	120 Jan. 31.	108 Jan. 13.	60 Jan. 23.	95 Nov 18
Difference	74	79	75	100	82	55	67

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A Table, in Inches and Decimals, of the Depth of Rain which fell in several Places, in the Year 1731, communicated to the Royal Society.

1731.	Cran- Court.	South- wick.	Upsal.	Witten- bergs N. S.	Naples.	Padua.	Edin- burgh.
January	125	81	574	1 557	2 60	2 546	
February	82	1 04	330	934	2 8	3 093	
March	05	15	2 544	1 775	478	976	
April	1 26	2 07	587	801	2 478	3 434	
May	30	33	669	2 610	1 130	602	
June	2 30	3 38	3 074	1 616	870	4 253	2 05
July	2 085	1 65	2 681	1 513	347	3 402	54
August	1 73	1 54	1 402	222	3 239	7 372	1 85
September	55	1 47	1 913	1 898	213	2 216	2 02
October	1 36	1 34	1 171	1 068	3 0	4 354	1 47
November	1 53	1 49	460	2 699	4 04	1 653	1 42
December	1 40	2 30	397	1 927	6 76	306	3 12
Total.	13 60	17 57	16 002	18 620	27 955	34 207	

The

The Barometrical Table for the Year 1732.

1732.	Crane-Count.	Southwick.	Kent. Lat. 51.	Coventry.	Wittenberg.	Naples.	Edinburgh.
Jan. 1	20 55	29 54	29 53	29 27	29 54	29 08	29 33
February	87	65	61	38	53	1	4
March	76	57	52	33	35	14	6
April	71	52	04	27	43	28 95	5
May	70	50	36	26	54	09	5
June	96	71	71	32	54	97	8
July	91	65	55	46	54	29 38	7
August	95	70	66	52	64	03	6
September	91	63	10	47	64	22	3
October	58	68	27	15	39	10	8
November	30 00	75	89	88	47	24	8
December	29 71	47	48	28	52	10	8
Mean of the whole Year.	29 82	29 627	29 53	29 37	29 51	29 1	29 60

21 High tide	30 55 Feb. 14	30 13 Nov. 24	30 29 Sept. 25	29 96 Nov. 25	30 25 Dec. 9.	29 40 Jan. Sept. 31	30 Aug. 28.
Lowest	30 00 Dec. 31	8 70 Dec. 30	28 32 April 5	28 54 Dec. 30	38 5729 Nov 17.	28 82 May 28.	28 22 Nov. 18.
Difference	1 55	1 43	1 97	1 42	1 524	58	2 38

The

The Thermometrical Table for the Year 1732.

1732.	Crane-Court.	Southwick.	Wittenberg.	Naples.	Edinburgh.
January	66 1	67 5	86 5	53 5	75
February	53 8	57	67 4	43	65 5
March	57	58 5	66 8	39 8	65 5
April	47 9	52	53 7	34 3	63
May	45 3	48 5	42 2	22 4	54
June	37 6	40	38 8	19 2	41 5
July	32 8	36 5	35 3	8 3	43
August	36	40	41 6	15 9	46
September	42	44	53 9	23	54
October	47 8	51	58 9	24 5	61
November	62 1	64 5	78 8	42	72
December	64 2	66	84 5	49 2	73 5
Mean of the whole Year.	49 3	52	58 8	31 3	59 5
Thermom					
Hight	19 Aug 20.	20 Aug. 13.	3 5 July 16.	4 July 17.	28 July 7.
Lowest	80 Dec. 12.	88 Dec. 11.	113 Dec. 11.	57 Jan. Sepe.	89 Dec. 14.
Difference	61	68	109 5	53	61

A Table of the Depth of Rain for the Year 1732.

1732.	Crane- Court.	South- wick.	Kent.	Witten- berg.	Naples.	Padua.	Edin- burgh.
January	525	9	537	399	570	2,129	1,28
February	190	12	276	1464	52	1959	2409
March	115	14	1216	917	33	2765	79
April	2765	12	1310	2514	130	5432	31
May	32	34	3494	1864	434	1864	462
June	105	6	803	2308	208	2872	119
July	113	18	1527	2929	120	1585	319
August	15	17	931	1390	113	3112	162
September	114	7	825	1833	78	089	000
October	239	37	3295	1346	470	9164	252
November	12	12	1269	1435	334	957	41
December	1705	26	1471	1524	482	3528	361
Total.	19555	205	18954	19923	26334	35456	2482

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The Barometrical Table for the Year 1733.

1733	Crane-Court	Southwick.	Kent.	Coventry	Wittenberg	Edinburgb
Jan	29, 06	29, 68	29, 65	29, 43	29, 57	29, 58
February	81	55	54	31	67	6
March	65	43	35	21	49	6
April	85	65	54	43	62	7
May	89	70	80	50	57	8
June	91	67	68	44	68	8
July	90	67	51	46	65	7
August	81	56	43	36	55	6
September	89	62	52	41	99	6
October	97	72	30 04	48	70	8
November	30 01	75	06	50	74	7
December	29 85	53	29 66	36	68	5
Mean of the whole Year	29, 87	29, 63	29, 65	29, 40	29, 67	29, 68
High it	30 55 Oct 19	30 17 Oct 19	30 52 Oct 19	30 1 Oct 18	0, 68 Sept 11	1, 0 Jan 18
Lowest	28 95 Mar 22	8 69 Jan. 1.	28 53 Sept 25	28 5 Feb 2	8, 62 Nov 12	28, 4 Sept 26
Differ	1, 6	1, 48	1, 1, 99	1, 51	2, 6	2, 6

The

The Thermometrical Table for the Year 1733.

1733	Crane-Court.	Southwick	Wittenberg	Edinburgh
January	60 1	61 5	78 8	70
February	59	60	70	68 5
March	59	59 5	68 6	68 5
April	52 8	51 5	53 6	59
May	45 3	47	50 5	51
June	35 2	36	38 3	41
July	28 5	32 5	36 1	38
August	35 9	39 5	41 7	47
September	45 1	48	57 2	54
October	53 4	56	72 5	62 5
November	56	58	68 7	62 5
December	54	55 5	68 9	62
Mean of the whole Year	47 59	50	50 24	57
Thermom Highest	16 June 26	10 June 25.	3 5 June 28	28 June 26.
Lowest	72 5 Jan 21.	95 Jan. 22.	99 Jan 19	86 5 Jan. 21.
Difference	56	75	95 5	58 5

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A Table of the Depth of Rain for the Year 1733.

1733.	Crane- Court.	South- wick.	Kent.	Witten- berg.	Padua.	Edinburg.
January	69	1 20	1 235	562	1 855	1 37
February	1 16	1 4	1 925	562	405	2 52
March	2 145	2 2	2 161	1 183	5 642	2 63
April	1 70	1	1 815	621	3 816	81
May	55	0 05	216	1 642	5 33	08
June	2 65	2	1 742	2 308	2 712	2 13
July	1 54	2 2	979	1 45	3 874	63
August	3 225	3 6	3 354	2 308	3 679	2 67
September	1 37	1 4	1 499	517	589	1 83
October	91	6	790	828	2 788	1 08
November	52	5	1 081	3 61	382	32
December	2 44	1 7	3 201	3 151	1 065	3 62
Total.	18 29	17 55	19 998	18 742	32 137	19 69

The

The Barometrical Table for the Year 1734.

1734.	Crane-Court.	Southwick.	Kent.	Coventry.
January	30 06	29 80	29 88	29 53
February	29 85	60	69	39
March	76	52	61	28 99
April	95	69	84	29 49
May	82	54	52	33
June	87	55	65	44
July	87	63	65	44
August	77	57	60	37
September	30 04	56	65	37
October	00	47	46	30
November	38	74	79	53
December	29 76	22	24	02
Mean of the whole Year.	29 92	29 58	29 63	29 43
The				
M m				
The				
g Highest	30 75 Nov. 28.	30 25 Jan. 12.	30 34 Jan. 11.	30 95 Jan. 12.
Lowest	28 7 Dec. 15.	28 10 Dec. 15.	28 19 Dec. 15.	27 9 Dec. 15.
Differ.	2 05	2 15	2 15	2 15

The foregoing Barometrical Table continued.

1734.	Darlington	Wittenberg	Nuremberg	Edinburg
January		29 38	28 58	29 39
February		72	96	6
March	29 40	51	34	5
April	6	59	97	8
May	30 10	54	65	8
June	29 8	63	42	8
July	8	56	66	7
August	9	60	63	6
September	7	37	71	6
October	8	07	18	5
November	3	66	71	9
December	8	54	5	0
Mean of the whole Year	29 87	29 56	28 363	29 64
2 Highest		30 35 Jan 11	29 2 Jan 22	30 6 Feb 12
Lowest		28 45 Dec 14	27 66 Dec 26	28 Dec 14
Differ		1 89	1 53	2 6

The

The Thermome ca. ab. fo Year 734

1734.	Crane-Court.	Southwick.	Darlington.	Wittenberg.	Edinburg.
January	67	68		81 2	75
February	58 7	57 55		66 3	68
March	51 7	54	60 6	6 9	61
April	47 6	50	51 1		51
May	45 3	47 5	46 3	12 0	8
June	36 9	39	40 2	5	13
July	34 7	35 5	35 6		11
August	34 6	37	37 3	34 5	46 5
September	45 6	47	45 1	19 5	58
October	50 4	55 5	55 6	71	61
November	61 1	64	60 3	56 6	72
December	63 5	65	63 1	76 7	74
Mean of the whole Year	49 3	52		57 55	58 55
Thermom	20 Aug. 8.	23 Aug. 23		9 June 23.	19 55 June 19.
Highest	17 5 Jan. 16	87 Jan. 10.		102 Nov. 27.	92 Jan. 13.
Lowest		64		93 55	72 55
Differ.	57 55				

A Table of the Depth of Rain for the Year 1734.

1734.	Crane- Court.	South- wick.	Kent.	Canter- bury.	Darling- ton	Witten- berg.	Rome.	Padua.	Edin- burgh.
January	1 01	35	1 63			86		1 03	59
February	1 935	2 6	2 43			2 04		1 73	59
March	1 79	1 8	1 98	74	3 25	2 95		1 55	2 12
April	45	6	59	75	1 7	55		1 7	1
May	4 17	5 1	3 89	1 49	2 1	3 7		4 37	3 31
June	3 21	1 3	2 13	3 59	2 4	1 67		4 55	2 21
July	1 11	1 8	1 4	1 94	1 6	2 47		7 01	7
August	1 76	+	2 16	2 34	2 4	1 09	3 59	3 08	1 28
September	1	1 7	1 59	1 54	1 35	2 01	5 83	2 89	1 17
October	2 1	2 8	3 13	3 94	2 4	2 08	5 83	4 39	1 32
November	1 77	9	1 49	1 61	2 3	73	5 15	1 3	1 6
December	4 27	4 4	7 26	6 52	1 7	75	6 02	4 9	2 33
Total.	24 57	27 5	29 572	23 98	21 2	20 95		38 56	19 22

The

The Barometrical table 735

1735.	Crane-Court.	Southwick.	Coventry.	Darlington	Nonington	Edinburgh.
January	30 09	29 57	29 53	29 51	28 53	29 55
February	1	63	42	4	7	7
March	29 84	36	15	13	11	3
April	30 03	49	26	15	40	7
May	14	60	42	30 7	51	8
June	11	56	35	29 92	63	
July	02	50	32	65	61	
August	31	72	46	92	83	
September	23	69	52	97	86	
October	19	69	44	30 10	72	
November	00	45	26	29 65	60	
December	13	59	37	60	1	
Mean of the whole Year.	30 09	29 56	29 34	29 55	28 52	
9 Highest	31 Jan 4.	30 51 Feb. 11.	30 Feb. 12.		29 53 Nov. 11	
Lowest	28 6 Jan 8.	28 00 Jan. 8.	27 59 Jan. 8		28 00 Jan. 10	
Differ	5 4	17	23 1		1 19	

N 11

7 be

The Thermometrical Table for the Year 1735.

1735.	Crane-Court.	Southwick.	Darlington.	Edinburgb
January	15 2.	65	62 8	73
February	64 3	62 5	66 3	74
March	55 1	60	56 2	69
April	49 7	50 5	49 3	61
May	40 5	48	45 2	54
June	39 5	41 5	39 1	
July	35 8	37 5	36 2	
August	34 6	28	34	
September	39 7	42	44 3	
October	55 9	56	56 2	
November	55 1	56 5	58	
December	61 5	62	64 2	
Mean of the whole Year.	50 11	52	50 9	
Thermom.				
Highest	21 Aug. 13.	21 Aug. 13.		
Lowest	76 Dec. 16.	81 Dec. 23.		
Difference.	55	60		

A

A Table of the Depth of Rain for the Year 1735.

1735.	Craue- Court.	South- wick.	Dayling- ton.	Padua.	Rome.
January	2 36	2 1	1 65	4 05	6 26
February	1 78	7	2 40	2 42	67
March	2 24	2 2	2 26	5 16	5 30
April	1 16	1 7	1 18	1 45	2 72
May	2 04	1 5	1 50	2 68	2 68
June	2 08	2 4	2 21	3 05	3 22
July	3 14	2 3	3 11	4 99	0 00
August	1 49	3 2	2 03	72	
September	1 56	3 2	1 04	1 28	
October	98	1 7	1 98	1 87	
November	2 69	1 7	2 87	51	
December	1 5	2 1	1 84	63	
Total.	22 83	25	24 57	29 68	

N n 2

IX. A

IX. *A short Account by James Parsons, M.D. F.R.S. of a Book intituled, Traité des Sens, &c. by M. le Cat, M.D. F.R.S. printed at Rouen, 1740. 8^{vo}.*

Read Dec. 16.
1742.

THIS Treatise appears, by the Advertisement prefixed to it, to be a Part of a Physiological Work, which the Author says is not likely to be soon published; and that he has therefore exhibited this Part for the Use of the Curious, and Lovers of Philosophy, who might not be so agreeably entertained by the rest of the Work, as treating chiefly of the Human Body, and therefore calculated rather for those of the Faculty of Medicine.

He begins the Book with Page 201. and says he has, before, established certain general Principles of *Sensation*, and that now he proceeds to recount the particular Parts with which Nature has furnished the animal Oeconomy, serving to our different Senses; and then expatiates a little upon the general Utility of them.

His First Chapter treats of the Sense of *Feeling*, wherein he has compiled all the different *Phænomena* that regard this Sense, as those of Heat, Cold, and other Objects of Feeling, with the Structure of the Skin, to which he thinks fit to subjoin Two known Histories, one of a blind Organist in *Holland*, who distinguished all kinds of Coins, and played at Cards, by Feeling; and the other of the famous Statuary *Ganibasius*, who, though stone-blind, could
by

by Feeling make a Statue in Clay, perfectly like what he felt. Our Author adds something of *Tickling*, and endeavours to prove, that Imagination has a great Share in the Cause of this Sensation, as well as the others; and thence he falls upon an Account of another Sense, which he brings under this Head; which he calls, *la Chatouillement de l'Amour*, of which he gives a florid Definition.

Tasting is his next Subject, wherein, as in the foregoing Chapter, he has drawn together the several Sections relating to it, as, an Account of the Organs of Taste, the Mechanism of *Savours*, and the manner of their being varied into compound Tastes. His Comparison here is new; he says, Since the Principles of Savours are Salts, both fixed and volatile, that Water, Earth, and Sulphur, serve to make the great Variety, and different Kinds, that are in Taste, just as Shadows variously mingled with Light form different Appearances; not that the Shadow is capable of making an Impression upon our Organs of Sight, but the Light alone; as the Salts alone are, upon our Organs of Taste. He has also some Reasoning upon the Difference that is in Mens Appetites to some Eatables, which were before disagreeable. His Reason is, not that the Organs differ at any time from what they always were, but because the Soul sometimes changes her Ideas, even from the same Impressions, and that therefore there can be no Ideas essential to any Impressions; or at least, that there are none which the Soul cannot change: He also says, that Imagination is much concerned in the Variation of Tastes.

The Sense of *Smelling* is discussed in his Third Chapter, wherein he observes the same Method as in the Two former, in describing the Mechanism of the Organs serving to that Sense, and accounting for the Conveyance of Odours to those Organs; and for the *Stimulus* of some odoriferous Particles causing Tears to flow, as well as Sneezing caused by a glaring Light; and, after making some Reflections on the many Effects of Smells upon the Human Body, and the exquisite Sense of Smelling in some Animals, he recites the Story told by Sir K. Digby, of the Boy brought up in a Forest, whose Smell was so exquisite as to perceive the Approach of Enemies, and warn his Parents of them. Our Author found this Story elegantly told, and reasoned upon, in *Monsieur Verduc's* Book called, *Usage des Parties*. He also mentions the Perfection of Smelling in the Inhabitants of the *Antibes*, who can run a Man upon the Nose like an Hound; and concludes this Section with a Relation of a *Frier* of *Prague*, from the *Journals des Scavans*, who could not only distinguish different Persons from each other by Smelling, but also an incontinent Woman from a chaste one; and adds, in a joking Strain, that this Man had begun a Treatise of Odours before he died, which the Journalists much regretted the Loss of: But, says *Monsieur le Cat*, for my part, I do not know but a Person so exquisite in this kind of knowledge would be dangerous in Society.

He proceeds next to treat of *Hearing*, and brings under that Head the whole Mechanism and Doctrine of Sounds; the Vibrations of all sounding Bodies: And from the Experiment of holding a Candle near a vibrating or sounding Body, without the Flame's
being

being moved or otherways affected, he argues, that the common Air does not produce the Sound, but a more subtil Fluid better proportioned to the Organs of Hearing: Here he runs into a Detail of the Principles of the Chords and Tones of Music, and makes a new and curious Comparison between the principal Colours in the Rays of Light, and the fore-said Fluid, which is more or less subtil in the Air, some Particles of which are only capable of being moved to express low Tones, others higher, and so on successively, as far as the Compass of Music reaches; just as the Light is composed of certain kinds of Rays, some of which produce *Red*, some *Green*, &c. This being supposed, says he, it may be conceived, that every Tone will move the Fluid that is proper to itself; and by that means the Ear may receive at once the Impressions of every Fluid, as the Eye receives the Impulsions of several coloured Rays at the same Instant. He adds to this, by way of Reasoning, that when a single String of an Instrument is touched, though the generality of Mankind can distinguish but One Tone, which he calls the *fundamental Sound*, yet People accustomed to Harmony can distinguish, besides, an *Octave*, a *Fifth*, and a *Third*, covered by this *fundamental Tone*; for the *Octave* is half that Sound, or the Produce of half the String; the *Fifth* is the Produce of Two-thirds, and the *Third* is the Produce of Four-fifths of the same String.

He proceeds to reason upon this in an agreeable Manner, and concludes his above-mentioned Comparison to this Purpose: Thus there are in the vibrated String all the Harmonies or Chords at once, which

which compose the *fundamental Sound*, by vibrating each its particular proper Fluid at the same time; just as the Assemblage of all the different primitive coloured Rays meeting together, makes the white Colour or Light: And so the Ear of a good Musician is a kind of *Prism*, which can separate and distinguish the Sounds or Tones from each other in the *fundamental Sound*. He gives an anatomical Description of the Organs of Hearing; and has added some good Figures of the external and internal Parts of the Ear, with the *Eustachian Tube*, much after the manner of *Du Verney*.

He has also the Figure of an Instrument, Page 292. to help those that are hard of Hearing, which he claims the Invention of. The particular *Form* of this Instrument may be new to the Author; yet we have had of this Kind in Use many Years in *England* for the same Purpose. He finishes this Section with some Reflections upon a young Man of a Town called *Chartres*, who was born deaf and dumb, and whose Hearing suddenly came to him, and who spoke some Months after. In this Place he has a very good Figure of the *Basis Cerebri*, by a transverse Section through the *Frontal Sinuses* a little above the Eyes, and continued through the *temporal Bones*; demonstrating the *Originations* and *Exit* of the Nerves, with the Conjunction of the *vertebral* and *carotid Arteries*, according to the Dissection of the famous *Willis*; and then proceeds to his last Section, which treats of *Seeing*.

This Section, in a word, is on the Structure of the Eye, and all the *Phænomena* of Vision. He begins it with the Doctrine of *Lights* and *Colours*, making
 use

use of many Experiments and Explanations of the great Sir *Isaac Newton*; having also added several of his own, besides some little Cavils, a mere *Jeu des Mots*, against that great Man's Doctrine of *Attraction*, to which he prefers the *Impulsion* of *Cartesius*. He quotes against Sir *Isaac*, *M. de Fontaine*, *M. Bannier*, and *M. Voltaire*; and as our young Author had a mind to oppose the Opinions of one of the greatest Abilities in the Sciences, common Prudence should have informed him, that the Name *Newton* bespeaks the greatest Modesty and Diffidence in the Attempt. Our Author amuses himself thus against that *Prince of Philosophers*, which is the more strange; since if he had wrote nothing on the Subject, *Monsieur le Cat* would have wanted a great Part of his Furniture for this Section.

The principal Authors besides, regarding Anatomy and Physiology, which our Author seems to have had in his View, are *Du Verney*, *Willis*, *Synac* upon *Heister*, and *Verduc's* excellent Book *L'Usage des Parties*. However, this *Treatise* of the Senses is judiciously compiled; nor does it want several ingenious Embellishments from the Author, besides the Opinions of several others; we may therefore conclude it to be a very useful Book.

X. *Observationes variæ Medico-Chirurgicæ a*
Johanne Daniele Schlichting, Med. & Chir.
Doctore, Acad. Cæsareo-Leopoldin. Nat.
Curios. Membro, & Commercii Literarii
Norimberg. Socio.

Circa Spinam Ventosam Animadversa nuper detecta.

Read Dec. 23.
 1742.

1. **C**OMPERII illam esse ægritudinem,
 quæ luem Veneream multum
 æmulata & humores & vasa corporis inficit.

2. Eam non semper ossa, neque medullam primum
 afficere, sed interdum quoque adipem, membranas, &
 posthac ipsa demum ossa.

3. Periosteum aliæque ambientes partes subinde solum
 apparent tumefactæ, quibus discissis nulla nondum
 ossis infectio.

4. Aliis os primum tumescit, idque in epiphyse.
 Digitorum autem ossa per totam suam molem ali-
 quando tument, unum aut plura.

5. Quando os carie infectum vidi, expertus sum
 insuper interdum ulcera, fistulas, tubera ad clunes, sub
 axillis, oculos aliasque partes vel inflammatas vel ex-
 ulceratas esse; quæ vero cuncta symptomata cedebant
 penitus unico remedio, Mercurio, huic morbo atque
 lui Veneris proprio.

6. Abscessus & ulcera ad articulos, ad periosteum
 & ligamentum usque penetrasse, neque ulla tamen
 caries conspici poterat. At subinde nuditas ossis
 comparet absque carie, aliquando cum carie ac sine

exostosi, & iterum exostosis absque nuditate & absque carie.

7. Facta suppuratione, exostosis postmodum adhuc multum resolvitur proprii remedii usu.

8. Immo, separatis ossium particulis reliquus osseus tumor pedetentim resolutione fatiscit.

9. Privantur ægri & mutilantur digito, manu, pede, vel uno aut multis articulis, neque malo tamen penitus orbantur: restat subtilis materia venenata humoribus mista, quæ prioribus similia mala postmodum licet aliis in locis resuscitat. Hinc pars aliquando frustra amputatur, quum vel ad eandem partem, aut proximam aut dislitam quoque, virus efficax appulsum eundem morbum excitare conspicitur.

10. Immerito hic loci, in civitate adeo populosa, soli naturæ miseri relinquuntur, per annos decem & ultra sic frustra afflicti, sperato quotidie vano naturæ spontaneo adminiculo. Expectando autem sat diu ac nimium, pereunt tandem tabidi quam miserrime ita malo vexati.

11. Sine fructu igitur multis futilibus atque vanis adhibitis remediis, medicatus diu vexans ægros liquorum decoctis, alvum sudoremque cientibus diureticis, terreis, minerali Æthiope, Cinnabari, Marte, omnibus ne hilum quidem proficientibus, consului **MERCURIUM**, & efficacem inveni, illum quidem præcipue extus affectæ parti illitum; quamvis & Panacæ forma intus ore assumtus ille mihi nonnunquam satis proficiuus & efficax obviis factus sit.

12. Partem tumentem, in quocunque sit statu, cute tectam aut ulceratam, cum vel sine ossis nuditate aut carie, unguento Neapolitano inungendam jubeo.

& *Ung. Ros.* ʒj. *Mercurii viv.* ʒ ss. *Terebinth. clar.* ʒj. M. F. *Ung.* ter, octo, decem aut quatuordecim dies pro re nata, & ætatis ratione, plus minus, citius tardiùve id oblinatur, imposito supra communi emplastro diapalm. defensivo, aliove: illinatur autem singulatim parumper bis de die: exhibito interdum, ad avertendum ptyalîsmum, leni pharmaco alvum ciente: Idque ad duos tresve menses aut ultra agendum est, donec ulcera funditus coaluerint, atque ossî tumores maximam partem disparuerint, desistendo interea nunquam ab usu Herculis, Mercurii, qui longo temporis spatio hic medetur tarde, non quidem per ptyalîsmum, contra ac in lue Venerea accidit. Tam enim parvo tempore omnia spinæ phænomena haud disparent salivæ fluxu, uti contra Syphilidi contingit.

13. Sæpe & fere semper vidi, hac sola methodo non modo tumores fuisse resolutos, verum & ulcera profunda, & ista cum carie putrida, fungosa, quæ fistulas mentiuntur, &c. penitus solidata, nulla omnino facta scissione, terebratione, abrasione, ustione; adhibita interim extus solummodo simplici ulceribus propria deligatione.

14. Ast ubi hac medendi ratione per aliquot hebdomadas porrecta comperii, haud ita faustum fore successum, quum pus, ichor, lymphæ intus reclusæ, aut ossis corrupti fragmentum vel separari vel excerni nescit, partem rite incidendam, atque spongia aut linteo'o carpto dilatandam satis curavi; adhibitis posthac vulneri *Myrrhæ* seu *Euphorbii* tinctura, *Aq. dir.* Fernel. *Ung. fusc. Fel. W. Alum. crud. & ust. Merc. præcip. & dulc. Spirit. Terebinth.* aliisque omnibus pro re nata in usum vocatis, quibus ulcera
for-

fordida una cum carie mudentur, caro excrescens fungosa coerceatur atque fauciatum consolidetur. Non neglecto tandem vinculo arctis fasciis stricto, una cum rara sub finem necessaria deligatione.

15. Tumorem vero partis solidaræ posthac sæpe numero diu residuum naturæ discutiendum relinquo, aut gummosum ei emplastrum affigendum curo.

Pleuritis suppurata per vulvam puellæ excreta.

Puella, vicini Ianionis filia, tres annos nata, afficitur pleuritide, quæ in abscessum vertebatur clausum, juncto per menses asthmate. Sed subito puris fluxus oboritur per puellæ vulvam, extra tempus lotii reddendi, idemque fere continuus, die noctuque per quatuor ferme menses haud interruptus, protractus, ut femina ac femora deglubarentur.— Quo elapso tempore ægrota perfecte convaluit, nullis pene adhibitis medicaminibus, & adhuc annis quinque post validam degit vitam.

Abscessus Lienis per vulvam facta puris excretionē sanatur.

Uxor J. Z. S. juvencula ante sex annos afficitur lienis tumore, quo disparente, per menses pus e vulva copiosum absque urina stillatim profluxit, & perfecte liberata est etiamnum vegeta ac valens.

Loco feminis sanguis expellitur in coitu.

Juvenis 26 annorum quatuor ante annos virulenta gonorrhœa afflicta, per annum usus præter pharinaca purgantia fortissimis diureticis, cruorem tandem loco
sc-

feminis ejaculari coepit absque dolore, cumque ex rufo nigricantem, nullum extra coitus aut pollutionis tempus passus ex urethra fluxum. Et licet per septimanam, immo per mensem unum aut plures, hinc omnino vitaverit excretionem, eundem post equidem ejaculatus est cruorem: neque vel semel quidem ad hoc usque tempus purum semen emisit, immo vix unquam aliquid albi semen emulantis rubro mixtum apparet. Fortissimis adstringentibus & temperantibus intus & extus utendum curavi, quando rubrum & nigricans parum albesceret. Posthac ptyalissimum institui protractum satis, aliisque in usum vocatis, nihil tamen cunctis proficientibus. Quare immedicabilis ille nobis declaratus est soli naturæ relinquendus. Numne in prostaticis aut vesicularum feminis altera ulceratis vas sanguiferum ruptum neque solidatum, ut causa, quærendum?

Coxæ articuli suppuratio, cum secessione capitis femoris, solidata, vide TAB. Fig. II.

Anno 1730. Puella rustica 14 annorum coxæ articulus tumescit, dolet, suppuratur, perrumpitur. Chirurgus dilatat foramen natura factum: extrahit totum ossis femoris caput: subjicit posthac in ulceris cavitatem *Myrrhæ tincturam*, porro *suscum ung. fel. IV.* stringit cam denique arêlo vinculo, raro deligat, atque sex septimanarum curriculo consolidat, ut puella postmodum libere liceat manca inesse. Ecce figuram ab isto Chirurgo ruditer delineatam

- Nº. 1. denotat acetabulum innominati ossis.
 2. caput ossis, quod ex ulcere extractum est.
 3. cruris collum, &c.

Aneurisma falsum non pulsans cruorem continens liquidum.

Anno 1741. conveni cum Chirurgis foeminam A. C. Carsteus, cui erat brachium dextrum, circa cubiti ac radii articulum cum ipso, prætumidum, pollicibus 32 crassum, a phlebotomia ante annum ibi facta, exortum, nunc inflammatum, rubicundum, valde dolens. Tumor erat extensus ab infima humeri parte ad carpum fere usque, uti TAB. Fig. III. demonstrat. Ad latus internum apparuit ulcusculum lividum gangrænæ junctum, rupturam præfagiens. Tactu erat instar vesicæ humoribus plenæ ac contractæ absque omni vel minima pulsatione, adeo durus ac densus, ut nullam, & re minimam quidem representationem admittēret. Ad carpum quoque nulla pene pulsatio micabat. Nonnulli putabant fungum adiposum clausum forte subesse, qui clausus æque durus comparet: ast ego cum aliis aneurisma non pulsans latere suspicati sumus. Hinc convenimus, omni brachio fasciis commode circumvincto, rupturam esse expectandam, quæ die etiam sequente tertio contigit: Quando, soluta ligatura, momento temporis, maxima vi cruor liquidus ultra libram profiliit. Chirurgus beneficio fungi bovis. & fasciis sedavit extemplo hæmorrhagiam. Duas post horas consensimus omnes, aut brachium amputandum, aut arteriam esse circumligandam. Elegimus autem arteriæ ligaturam, apposita primum machina torculario sive *turnequet* dicta ad arteriam supra comprimendam, discissilis supra aneurisma, in parte sana, brachii ferme media, cute adipe ac ventre bicipitis muscoli, atque acu cum
filo

filo infra aut subtus arteriam transmissa, ac filo circumligato. Tum, sursum ac deorsum omni diffusio aneurismate, sanguis, isque clarus, rubicundus, liquidissimus, copiosus, vel ad libras quatuor subito profluxit, quem ceperat ipsius una cavitas. Mirum spectatu, hic nullum repertum fuisse arteriæ saccum, uti in vero aneurismate videre est, neque polypum aut sanguinis grumum, ut in falso, sed cavitatem longe aliam inter cutim & musculos atque inter musculos ipsos formatam, atque musculos ad brachium inferius sitos omnes a se invicem sejunctos, ac si arte essent separati, sed pallecentes, vapidos, luridos, ad quos adhærebat parca quædam gelatina mucii mollis instar, quam digitis ipse deterfi, & quæ elota albescebat penitus: in memorata cavitate porro mirabundi omnes conspeximus ex sex septemve locis supra infra, & e lateribus, tanquam ex tot canaliculis, cruorem vi atque copia profiliisse. Ad eos tum canaliculos obstopandos admovimus fortissima styptica, cum *bovisf. boleto, vitriolo, spiritu terebinthinæ, alcohol Vini*, &c. quibus & omnem cavitatem ope lintei carpti implevimus, arctis emplastris atque fasciis circumvincto supra vinculo ista: quidem cito sanguinis flumen coercitum est, ast mala ex malo exorta symptomata, qua rupturæ præcesserant, uti erant febris, ructus, nausea, vomitus, singultus cum calore, lipothymia, tendinum subsultu, &c. vesperi exasperabantur. Die sequenti iisdem crescentibus, juncto delirio, tertio die mane, non obstante temperantium, anodynorum, cardiacorum usu sat largo, vitam cum morte commutavit. Et quum raro admodum ejusmodi exemplum occurrit, ideo memoratu dignum

id

id judicans huc transcripsi, figuramque hanc tertiam apposui. Notantur autem in TAB. Fig. III.

- N^o. 1. Brachii dextri aneurismate obstiti interius
latus.
2. Locus unde sanguis sponte prorupit.
3. Brachii dextri ejusdem externum latus.

Scripseram *Amstelædami*,
d. 16 Septembr. 1742.

XI. *Observationes duæ Anatomico-practicæ, una de Infante nato cum Sacco Aqua pleno, ab Offe Sacro usque ad Talos propendente; altera de Hydrocephalo singulari. Auctore Job. Baistero, M. D. R. S. S.*

Read Dec. 23.
1742.

OMNE illud, quod lymphatica vasa ita obstruit, ut transitus contentæ lymphæ ad cor impediatur, hydropem causari potest: sic quando in partu difficili caput nimium premitur, vel capite jam nato, os uteri collum, vasaque jugularia ita constringit, ut reditus sanguinis per arterias vertebrales allati inde impediatur, oriri potest Hydrocephalus.

Sic *Lowerus*, libro de corde, Cap. I. in cane vinculo ligabat venas jugulares, arteriis aperte relictis, viditque canis caput pedetentim intumescere, ipsumque hydropicum fieri: & ipse in suspensis hominum cadaveribus plus semel expertus sum, laqueo reditu sanguinis ex capite impedito, cavitates cerebri humore aquoso esse repletas, plexumque choroideum hydatidibus scatere.

Aut oritur hydrocephalus, quando infantes cer-vice incurvo prodeunt, aut ab obſetricibus nimis rude tractantur, aut ante juſtum tempus prono capite in utero prolabantur. Multum huc facit Phlegmaticum matris temperamentum, & victus ex crudis & concoctu difficiliſſimis.

Quoniam ergo in omni corporis puncto inveniuntur venæ, vehentes lympham ſanguine tenuiorem, hinc ubique in corpore hydrops oriri poteſt; qui, ſi univerſalis, anafarca, ſi particularis, a partibus affectis, ut hydrocephalus, hydrops peſtoris, uteri, ovarii, ſcroti, &c. nomen habent.

Cum vero hoc anno rareſſimum hydrocephali & hydropis ſaccularis, ut loqui liceat, in praxi mea caſum obſervare licuit, illos ad vivum coloribus depictos Regiæ Societati offerre veniam rogo.

Primus caſus fuit infantis maſculini ſexus, cui in tergo, ubi os ſacrum eſt, oriebatur ſaccus uſque ad calcaneos propendens, vera cutis productio, intus, ut tangenti videbatur, humore aquoſo plenus: hic infans, quamvis roſco ſuo colore videbatur ſaniſſimus, paucos dies ſupervixit: mortuum diſſecare non licuit, ſed ad vivum depinxit, omnibus partibus menſuratis, accuratus pictor.

Alter infans duos annos cum ſemiſſe natus erat, eo ipſo quo moriebatur die, in omni quo tempore niſi maternum lac hauſit. A patre ſaniſſimo procreato, ſed a matre cachectica in lucem edito, caput quidem ipſi erat juſto majus, ſed pedetentim hoc ad delineatam excrevit magnitudinem, (licet parentes plures medicos & chirurgos conſuluſſent, plurimaque, ſed incaſſum, tentaſſent remedia) ita ut teneræ vires non-

monstruosum hoc caput sustinere essent impares, infanque semper jacere cogeretur.

Qui caput hoc post mortem filo metiebatur, ab auditorio meatu dextro supra ossa bregmatis ad meatum sinistrum filum viginti & semipollicem Rhénolandicos longum reperiebat: qui a radice nasi incipiens, filum ducebat ad primam dorsí vertebam, huic viginti tales pollices filum erat: qui vero a radice nasi incipiens uno circuitu occipitis, frontis & temporum ossa cingebat, ejus filum viginti & quinque pollices Rhénolandicos longitudine superabat. Itá hujus capitis ossa a se invicem erant distenta, intra cujus integumenta communia nil feri aut aquæ tamen fuit repertum.

Aperto autem secundum artem cranio, & prudenter elevata dura matre, apparebat pia, sed tenerrima, sed pellucidissima plena humore aquoso,—nullo odore, nullo sapore prædito, sed & ita pellucido, ut per eum, tamquam crystallum, ad fundum cranii posset perspici.

Ad fundum cranii, inquam; ipsa enim cerebri substantia, quam inter alia anatomica conservo, ita erat compressa, ut nil minus quam cerebrum videbatur; sed tantum firma membrana, quæ in his locis crassior, in aliis erat tenuior.

Tres cerebri cavitates unam modo cavitatem formabant, ubi medulla oblongata & cerebellum, sed incredibiliter parvum, videri erat. Nates, testes, anum, aut cerebri vulvam, aut ullas ejus protuberantias, aut medullam spinalem frustra quæsieris, nec vestigia ipsa apparebant.

Contentus vero humor prudenter effusus & receptus replebat quinque pintas, ad stateram examinatus sex libras & undecim ponderabat uncias.

Dum infans ille vivebat, non nisi vitales ac naturales actiones in eo videbantur, animales vero nullæ: quietus semper erat, non lacrymans, semper quasi dormiens, surdaster, & sine ulla convulsione, aut motu sensibili, obiit.

Printed for T. WOODWARD, at the *Half-Moon*,
between the *Two Temple-Gates* in *Fleetstreet*;
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Holbourn; PRINTERS to the ROYAL SO-
CIETY. M.DCC.XLII.

E R R A T A.

Pag. 227. l. 13. for two or three more, read two, three or more.

N. B. N^o 467. being paged from i—xviii. which answers to pag.
281.—298. we shall begin N^o 468 with p. 299.

S O M E
P A P E R S
Lately Read before the
ROYAL SOCIETY
Concerning the
Fresh-water POLYPUS;

A N
I N S E C T, which hath this surprizing
Property, That being cut into several Pieces, each
Piece becomes a perfect Animal, as complete as
that of which it was originally only a Part.

Collected and Published by
CROMWELL MORTIMER, *M. D. &c.*
Secretary of the ROYAL SOCIETY.

L O N D O N:

Printed for T. WOODWARD, at the *Half-Moon*,
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CIETY. M.DCC.XLIII.

PHILOSOPHICAL TRANSACTIONS.

For *Thursday*, January 13. and *Thursday*, January
21. 1742-3.

The CONTENTS.

*Several Papers relating to the Fresh-water
POLYPUS, an Insect, which has this surpris-
ing Property, that being cut into several
Pieces, each Piece lives, and in a short time
becomes as perfect an Insect, as that of which
it was originally only a Part.*

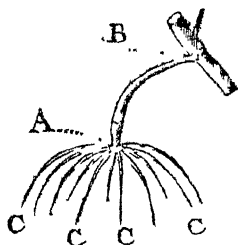
Abstract of Part of a Letter from the Honourable William Bentinck, Esq; F. R. S. to Martin Folkes, Esq; Pr. R. S. communicating the following Paper from Mons. Trembley, of the Hague.

S I R, *Hague, Jan. 15. N. S. 1743.*

‘ **W** HAT I here send you inclos’d will, I hope,
 ‘ answer the Queries of your last Letter.
 ‘ Mr. *Trembley*, the Gentleman who has
 ‘ made the Observations on the Insects, has drawn this
 ‘ Extract from his Journal: And I can answer for the
 ‘ Truth of the Facts therein contained, as there is not
 ‘ one of them but what I have seen repeated above
 ‘ Twenty times. I send you the Paper in *French*, not
 ‘ having had Time to translate it. I wish others may
 ‘ be encouraged to try the Experiments over-again,
 ‘ and satisfy themselves of the Truth, by their own
 ‘ Eyes. The Insects may certainly be found in *Eng-*
 ‘ *land*, if carefully lookt for, especially by such as
 ‘ are accustomed to such Enquiries. However, if that
 ‘ should be found difficult, it may be easy to send
 ‘ some over to you: And Mr. *Trembley* will give Di-
 ‘ rections how to keep and feed them; for he makes
 ‘ himself a *Point d’honneur* of being communicative,
 ‘ and concealing nothing of what he knows about
 ‘ them. If therefore you have any Doubt, or want
 ‘ any further Information, please only to write to me,
 ‘ or to him, and you shall be sure of an Answer, by the
 ‘ first Opportunity. I pray to be kindly remem-
 ‘ bred— &c.

The

The Figure of the Fresh-water Polypus, sticking to a Twig.



Observations and Experiments upon the Fresh-water POLYPUS, by Monsieur Trembley, at the Hague. Translated from the French by P. H. Z. F. R. S.

THE Animal in question is an aquatick Insect, of which mention is made in the *Philosophical Transactions* for the Year 1703. N^o. 283. Art. IV. pag. 1307. and N^o. 288. Art. I. pag. 1494.

It is represented in the Figure annexed. Its Body *AB*. which is pretty slender, has on its anterior Extremity *A*. several Horns *AC*. which serve it instead of Legs and Arms, and which are yet slenderer than the Body. The Mouth of the *Polypus* is in that anterior Extremity; it opens into the Stomach, which takes up the whole Length of the Body *AB*. This whole Body forms but one Pipe; a sort of a Gut, which can be open'd at both Ends.

The Length of the Body of a *Polypus* varies according to its different Species, and according to many other Circumstances, to be mentioned hereafter.

I know two Species, of which I have seen some Individuals extend their Bodies to the Length of an Inch and a half; but this is uncommon. Few are generally found above 9 or 10 Lines long; and even these are of the larger Kind.

The Body of the *Polypus* can contract itself, so as not to be above a Line, or thereabouts, in Length.

Both in contracting and extending itself, it can stop at any Degree imaginable, between that of the greatest Extension, and of the greatest Contraction.

The Length of the Arms of the *Polypus* differs also according to the several Species: Those of one of the Species that I know, can be extended to the Length of seven Inches at least.

The Number of Legs or Arms is not always the same in the same Species. One seldom sees in a *Polypus*, come to its full Growth, fewer than six.

The same may be said of the Extension, and of the Contraction of the Arms, which I have said concerning the Body.

The Body and the Arms admit of Inflexion in all their Parts, and that in all manner of Ways.

From the different Degrees of Extension, Contraction, and Inflexion, which the Body and the Arms of the *Polypus* admit of, results a great Variety of Figures, which they can form themselves into.

These Insects do not swim, they crawl upon all the Bodies they meet with in the Water, upon the Ground, upon Plants, upon Pieces of Wood, &c.

Their most common Position is, to fix themselves by their posterior End *B.* to something, and so stretch their Body and Arms forwards into the Water.

They

They make use of their progressive Motion, so as to catch their Prey. They are voracious Animals: Their Arms extended into the Water, are so many Snares which they set for Numbers of small Insects that are swimming there. As soon as any of them touches one of the Arms, it is caught.

The *Polypus* being seized of a Prey, conveys it to his Mouth, by contracting or bending his Arm. If the Prey be strong enough to make Resistance, he makes use of several Arms.

A *Polypus* can master a Worm twice or thrice as long as himself. He seizes it, he draws it to his Mouth, and what is more, swallows it whole.

If the Worm comes endways to the Mouth, he swallows it by that End; if not, he makes it enter double into his Stomach, and the Skin of the *Polypus* gives way. The Size of the Stomach extends itself so as to take in a much larger Bulk than that of the *Polypus* itself, before it swallowed that Worm. The Worm is forced to make several Windings and Folds in the Stomach, but does not keep there long alive; the *Polypus* sucks it, and after having drawn from it what serves for his Nourishment, he voids the Remainder by his Mouth, and these are his Excrements. According as the Weather is more or less hot, the *Polypus* eats more or less, oftener or less often.

They grow in Proportion to what they eat; they can bear to be whole Months without eating, but then they waste in Proportion to their Fasting.

The Observations related in the *Philosophical Transactions*, principally concern the Manner in which

which these Insects multiply. What is there said of them, is true and exact. The more one searches into the Manner how a *Polypus* comes from the Body of its Parent, the more evidently is one persuaded, that it is done by a true Vegetation.

There is not on the Body of a *Polypus* any distinguished Place, by which they bring forth their Young. I have some of them, that have greatly multiplied under my Eyes, and of which I might almost say, that they have produced young ones, from all the exterior Parts of their Body.

A *Polypus* does not always put forth a single young one at a time; it is a common thing to find those which produce five or six: I have kept some which have put forth nine or ten at the same Time, and when one dropt off, another came in its Place.

These Insects seem so many Stems from which issue many Branches. I have learned by a continual Attention to two Species of them, that all the Individuals of these Species produce young ones.

I have for two Years had under my Eye thousands of them; and though I have observed them constantly, and with Attention, I never observed any thing like Copulation.

Upon Supposition, that this Copulation is perform'd in some secret Manner: I tried at first to be sure it had not Place between two of them, after they were severed from the Body of their Parent. To this end, I took young ones, the Moment they came from the Parent, which was alone in a Glass; or I even parted them with Scissars: Each of these young ones I put into perfect Solitude, I fed them every one separately in a Glass, they all multiplied, not only them-

themselves, but also their Offsprings, which from Generation to Generation, as far as the Seventh, were all confined to Solitude with the same Precaution.

Another Fact, which I have observed, has proved to me, that they have the Faculty of multiplying, before they are severed from their Parent. I have seen a *Polypus*, still adhering, bring forth young ones; and those young ones themselves have also brought forth others. Upon Supposition, that perhaps there was some Copulation between the Parent and young ones, whilst they were yet united; or between the young ones coming from the Body of the same Parent; I made divers Experiments, to be sure of the Fact; but not one of those Experiments ever led me to any thing that could give the Idea of a Copulation.

The *Polypus* multiplies more or less, as he is more or less fed, and as the Weather is more or less warm. If plenty of Food, and a sufficient Degree of Warmth concur, they multiply prodigiously.

I now proceed to the Singularities resulting from the Operations I have tried upon them.

If the Body of a *Polypus* is cut into two Parts transversely, each of those Parts becomes a complete *Polypus*. On the very Day of the Operation, the first Part, or anterior End of the *Polypus*, that is, the Head, the Mouth, and the Arms; this Part, I say, lengthens itself, it creeps, and eats.

The second Part, which has no Head, gets one; a Mouth forms itself, at the anterior End; and shoots forth Arms. This Reproduction comes about more or less quickly, according as the Weather is more or less warm. In Summer, I have seen Arms begin to
sprout

sprout out 24 Hours after the Operation, and the new Head perfected in every respect in a few Days.

Each of those Parts, thus become a perfect *Polypus*, performs absolutely all its Functions. It creeps, it eats, it grows, and it multiplies; and all that, as much as a *Polypus* which never had been cut.

In whatever Place the Body of a *Polypus* is cut, whether in the Middle, or more or less near the Head, or the posterior Part, the Experiment has always the same Success.

If a *Polypus* is cut transversely, at the same Moment, into three or four Parts, they all equally become so many complete ones.

The Animal is too small to be cut at the same time into a great Number of Parts; I therefore did it successively. I first cut a *Polypus* into four Parts, and let them grow; next, I cut those Quarters again; and at this rate I proceeded, till I had made 50 out of one single one: And here I stopp'd, for there would have been no End of the Experiment.

I have now actually by me several Parts of the same *Polypus*, cut into Pieces above a Year ago; since which time, they have produced a great Number of young ones.

A *Polypus* may also be cut in two, lengthways. Beginning by the Head, one first splits the said Head, and afterwards the Stomach: The *Polypus* being in the Form of a Pipe, each Half of what is thus cut lengthways forms a Half-pipe; the anterior Extremity of which is terminated by the half of the Head, the half of the Mouth, and Part of the Arms. It is not long before the two Edges of those Half-pipes close, after the Operation: They generally begin at the
posterior

posterior Part, and close up by degrees to the anterior Part. Then, each Half-pipe becomes a Whole-one, complete : A Stomach is formed, in which nothing is wanting ; and out of each Half-mouth a Whole-one is formed also.

I have seen all this done in less than an Hour ; and that the *Polypus*, produced from each of those Halves, at the End of that time did not differ from the Whole-ones, except that it had fewer Arms ; but in a few Days more grew out.

I have cut a *Polypus*, lengthways, between Seven and Eight in the Morning ; and between Two and Three in the Afternoon, each of the Parts has been able to eat a Worm as long as itself.

If a *Polypus* is cut lengthways, beginning at the Head, and the Section is not carried quite through ; the Result is, a *Polypus* with two Bodies, two Heads, and one Tail. Some of those Bodies and Heads may again be cut, lengthways, soon after. In this manner I have produced a *Polypus* that had seven Bodies, as many Heads, and one Tail. I afterwards, at once, cut off the seven Heads of this new *Hydra* : Seven others grew again ; and the Heads, that were cut off, became each a complete *Polypus*.

I cut a *Polypus*, transversely, into two Parts : I put these two Parts close to each other again, and they reunited where they had been cut. The *Polypus*, thus reunited, eat the Day after it had undergone this Operation : It is since grown, and has multiplied.

I took the posterior Part of one *Polypus*, and the anterior of another, and I have brought them to reunite in the same manner as the foregoing : Next Day, the *Polypus* that resulted, eat : It has continued well these

two Months, since the Operation: It is grown, and has put forth young ones, from each of the Parts of which it was formed. The two foregoing Experiments do not always succeed; it often happens, that the two Parts will not join again.

In order to comprehend the Experiment I am now going to speak of, one should recollect, that the whole Body of a *Polypus* forms only one Pipe, a sort of Gut, or Pouch.

I have been able to turn that Pouch, that Body of the *Polypus*, inside-outwards; as one may turn a Stocking.

I have several by me, that have remained turned in this manner; their Inside is become their Outside, and their Outside their Inside: They eat, they grow, and they multiply, as if they had never been turned.

Facts like these I have related, to be admitted, require the most convincing Proofs. I venture to say, I am able to produce such Proofs.

They arise from the Detail of my Experiments, from the Precautions I have taken to avoid all Uncertainties, from the Care I have used to repeat the same Experiment several times, from the Assiduity and Attention with which I have observed them.

All this would require a Discussion too long to be here related.

I might also appeal to the Quality and the Number of the Persons who have been Witnesses to these Facts; as well of those who have seen *me* observe, as of those who have observed *themselves*.

For Brevity-sake, I have omitted several curious and material Facts.

If any Persons in *England* shall be desirous to make Observations on the *Polypus*, and to repeat my Experiments; I hope I shall be able to send some over, in case they shall not be found there.

They are to be look'd for in such Ditches whose Water is stock'd with small Insects. Picces of Wood, Leayes, aquatic Plants, in short, every thing is to be taken out of the Water, that is met with at the Bottom, or on the Surface of the Water, on the Edges, and in the Middle of the Ditches. What is thus taken out, must be put into a Glass of clear Water, and these Insects, if there are any, will soon discover themselves; especially if the Glass is let stand a little, without moving it; for thus the Insects, which contract themselves when they are first taken out, will again extend themselves when they are at Rest, and become thereby so much the more remarkable.

In order to feed them, one must know how to provide one's self with Insects fit for their Food.

If that is thought necessary, I will point out the Means I make use of for that Purpose.

I am ready to impart to every one who shall desire to make Observations on these Animals, all the Means and Contrivances I have used; to enable them to practise the same, and to judge of them.

I shall set forth all these Means and Contrivances in the History of the *Polypus*, which I am now at work upon. But if, before its Publication, any Informations should be desired, I again repeat, that I shall be ready and willing to furnish them.

An Abstract of what is contained in the Preface to the Sixth Volume of Mons. Reaumur's History of Insects, relating to the above-mentioned Observations, and delivered in to the ROYAL SOCIETY, immediately after the foregoing Paper.

AS the Sixth Volume of Mons. *Reaumur's History of Insects*, just publish'd at *Paris*, is not yet come over, so as to be met with among the Booksellers here; I thought a short Extract of what he there says, in his Preface, relating to the Subject of Monsieur *Trembley's* Paper, might not be unacceptable, to the Gentlemen of the SOCIETY present.

He there observes, that tho', in the Histories he has already given of minute Animals in this Work, he has had occasion to produce many new and unexpected Phenomena: One he has now to mention would exceed all Belief, if it was not confirmed by the strongest repeated Observations; which is, that there are Species of Insects, who are multiplied by being cut to Pieces, and among which, one single Animal, divided into 8, 10, 20, 30, or 40 Parts, becomes so many entire Animals, each similar to that of which it was at first only a Piece. This Animal, being one of those that undergo no Change in their common Form, does not belong to the Design of this present Volume, which treats only of some of those, which, having been first a Worm or Maggot, are then changed into a Chrysalis, and from thence, either into a Fly or Scarabee.

Yet

Yet *Monf. Reaumur* observes, the Number of Questions that had been put to him concerning this Insect, as well by Persons at home, as by his Correspondents abroad, had made him think he ought not to defer giving now some Satisfaction to the Curiosity of the Publick. Besides that he found himself obliged, to give in this manner his Attestation to the Truth of this Fact, first observed by *Mr. Trembley*, a Gentleman of *Geneva*, now residing in *Holland*, and confirmed by Numbers of the most curious and accurate Experiments: Which Attestation he also observes, in so strange a Fact, could hardly be expected to have sufficient Weight, should he not say enough to put his Readers in a Condition to observe themselves, and see with their own Eyes, the Truth of the Particulars he relates.

Mr. Trembley, about two Years and a half since, observing the numerous Insects, with which the Water of a Ditch, covered with Duck-weed, was plentifully stock'd, discovered some odd-shaped Bodies, of a greenish Colour, concerning which he was doubtful whether to look on them as Plants or Insects; he thought, by cutting them, to assure himself to which of these Classes they properly belonged; as supposing, if to the former, they would probably not be destroyed by cutting, but vegetate again: They seemed to do so; and upon this, he was inclined to look on them as a Sort of Water Sensitives; till fresh Experiments every Day shewed him new Operations. They discovered a sort of voluntary locomotive Faculty; they seemed to seek the Light; they caught other Insects, and they devoured them with great Eagerness. This threw him into fresh Amazement; yet a prudent Diffidence still hindered

hindered him from pronouncing positively concerning them; he communicated his Observations, and dispatched some of the Insects themselves to Monsieur *Reaumur*, in *December* 1740. He, he says, repeated all Mr. *Trembley*'s Experiments, and not only by himself, but with *Monf. Bernard de Jussieu*, of the *Royal Academy of Sciences*, and of this SOCIETY, and with several more of the *Academy*. The Experiments succeeded as they had done in *Holland*, and they were all convinced they could not refuse acknowledging the Insects in question, to be really such: however new and surprising their Properties appeared.

Monsieur *Reaumur* then gives a general and very succinct Account of the Experiments tried by Mr. *Trembley* and himself; which agreeing perfectly with what is contained in the Paper just read to the SOCIETY, I avoid again transcribing. I shall only add, that he says, when he, the first time, saw two complete Animals forming themselves from the Parts of one *Polypus* cut asunder, he did not know how to believe his Eyes; and that he can hardly yet see it without fresh Amazement, after Experiments a hundred and a hundred times repeated. He avoids, he tells us, entering into further Details of Particulars, as these will be seen in the Work Mr. *Trembley* is actually finishing upon this Head; the Publication of which, he says, would be expected with the utmost Impatience, if the Public could know, as he does, how many curious Facts they will there find; and with what Pleasure they will read the Account of the ingenious Means, by which that Gentleman has enabled himself, to come at the Knowledge of so many strange and singular Truths.

These Experiments were no sooner known among the Curious in *France*, but it was presently imagined, these Insects were not the only Species to which Nature had given so extraordinary a Faculty : And numberless Observations were made to that Purpose. Monsieur *Bonnet* was not long before he found a very slender Water-worm, of about an Inch and a half long, that had the same Property ; and Monsieur *Lyonnet* discovered another above three Inches long, and of the Thickness of the Treble String of a Violin, that being cut into 30 and 40 Parts, afforded the same Phenomena. Monsieur *Reaumur* was desirous, if possible, to see the Success of these Experiments in some Animals of a larger Size ; and inclin'd to believe some Sea Productions, not very unlike in Shape to these Fresh-water ones, together with other Bodies among those distinguished by the Name of *Urticæ Marinae*, and Star-fish, might not improbably be endowed with the like Faculties ; he engaged Monsieur *Guettard*, and Monsieur *Jussieu*, to be assistant in making Variety of Experiments by the Sea-side, on these several Sorts of Bodies ; the first was on the Coasts of *Poictou*, the other on those of *Normandy* ; and they were soon sufficiently satisfied, the same Laws of Nature had Place in these Sorts of Animals also. Many of the Star-fish Kind particularly, and which usually consist of five equal *Radii* or Arms, were found wanting, some one, others two, three, or four, of those *Radii* ; and Nature was reproducing in them the *Radii* wanting. Monsieur *Jussieu* broke and cut Star-fish into several Parts ; he had the Pleasure to see those several Parts continue alive, and to observe their Wounds to cicatrize and heal, though
he

he could not stay long enough in the Country to see the new Parts break out, in the room of those he had cut-away; which has been however, supplied by Monsieur *Girard de Villars*, who, on the Coasts near *Rochelle*, has seen the *Urtica* reproduce all that had been cut off, and the Parts of the Stars also putting out each new *Radii* in the room of those he had deprived them of. Monsieur *Jussieu* also reported, that this Fact in the Star-fish, so new to him, was not so to the common Fishermen of the Country, who seeing him tearing and cutting to Pieces one of those Animals, told him, *Qu'il auroit beau faire, qu'il ne parviendroit pas à leur ôter la vie*: Those poor People having been accustomed to see daily a Fact, the more philosophical Part of Men had never so much as heard of.

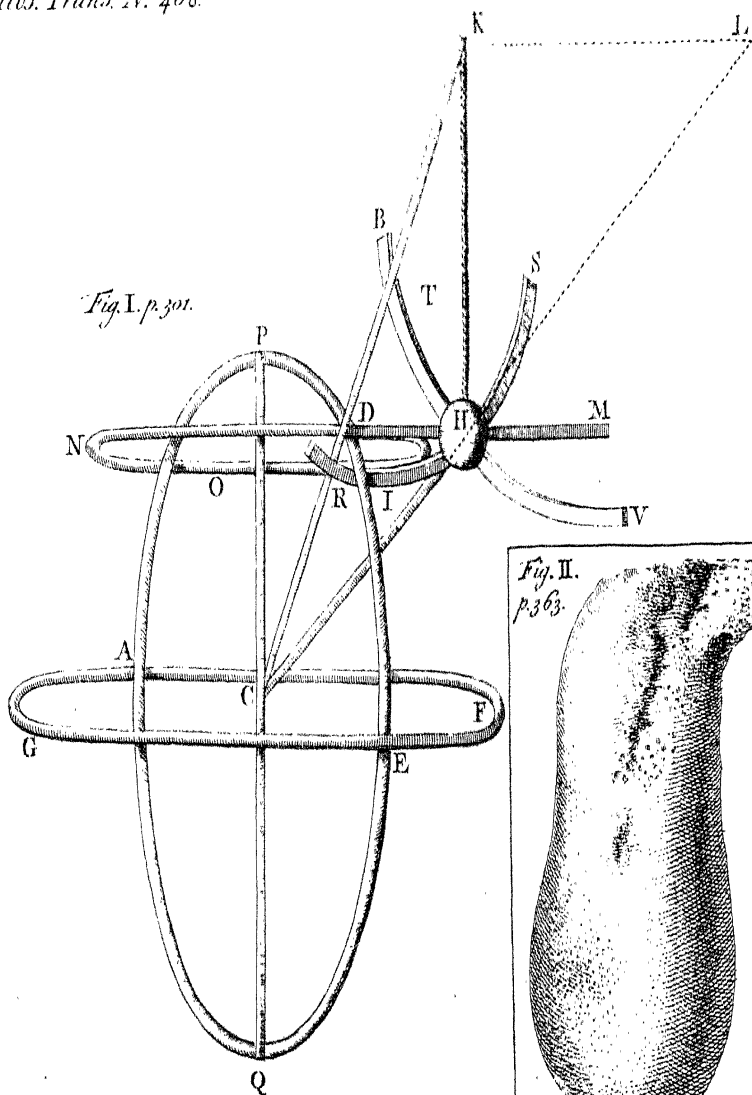
Monsieur *Reaumur*, though very sensible that Water Insects had a considerable Advantage over others for the Recovery of their Wounds, was yet willing to try if some Land Insects might not possibly afford also some like Observations; and after several Trials, both he and Monsieur *Bonnet*, have met with some Sorts of Earth Worms capable of bearing the Operation. Monsieur *Reaumur* has cut in two some of these Worms; the anterior Part, that to which the Head belong'd, seem'd to have little suffer'd: In less than two Days, the *Anus* was form'd again, as it had been before, and the Worms were complete to all Purposes, but that they were shorter, and wanted of their Length: They lengthened, however, by degrees, the Number of the Rings of which they are composed increased, and they came again to their first Length at the End of some Months. But the posterior Part of

the Worms, that to which the Tail belonged, wanted Matters of another Consequence; that Part had lost not a Head only, but the Parts of Generation also of both Sexes; which in those Creatures are placed not very distant from the Head: And to reproduce all these was the Work of some Months; it has, however, in many Instances, been done after that Time; and several of these hinder Parts of Worms have again become complete Worms, having each both a Head, and Male and Female Parts of Generation, and in as complete a Manner, as the whole Worms had before their Section. Tho' many, indeed, have died during the Operation, before the Reproduction was made complete: Yet, as in various Instances, where proper Care has been taken, the Experiment has succeeded; it is equally to be regarded, as if the making it required neither so long Time, nor so much Care.

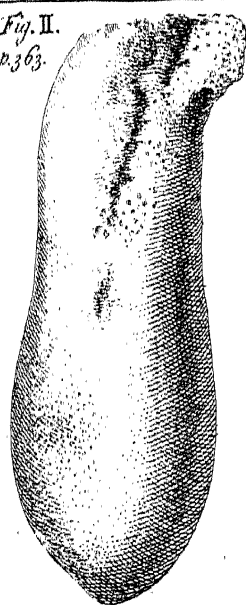
This general Account of what is relating to this Subject, in the Preface to Monsieur *Reaumur's* new Volume, I hoped might not be disagreeable to the Company, as it might in some measure gratify their present Curiosity, till this Preface itself, and Monsieur *Trembley's* own Book, shall be in every Body's Hands.

F I N I S.

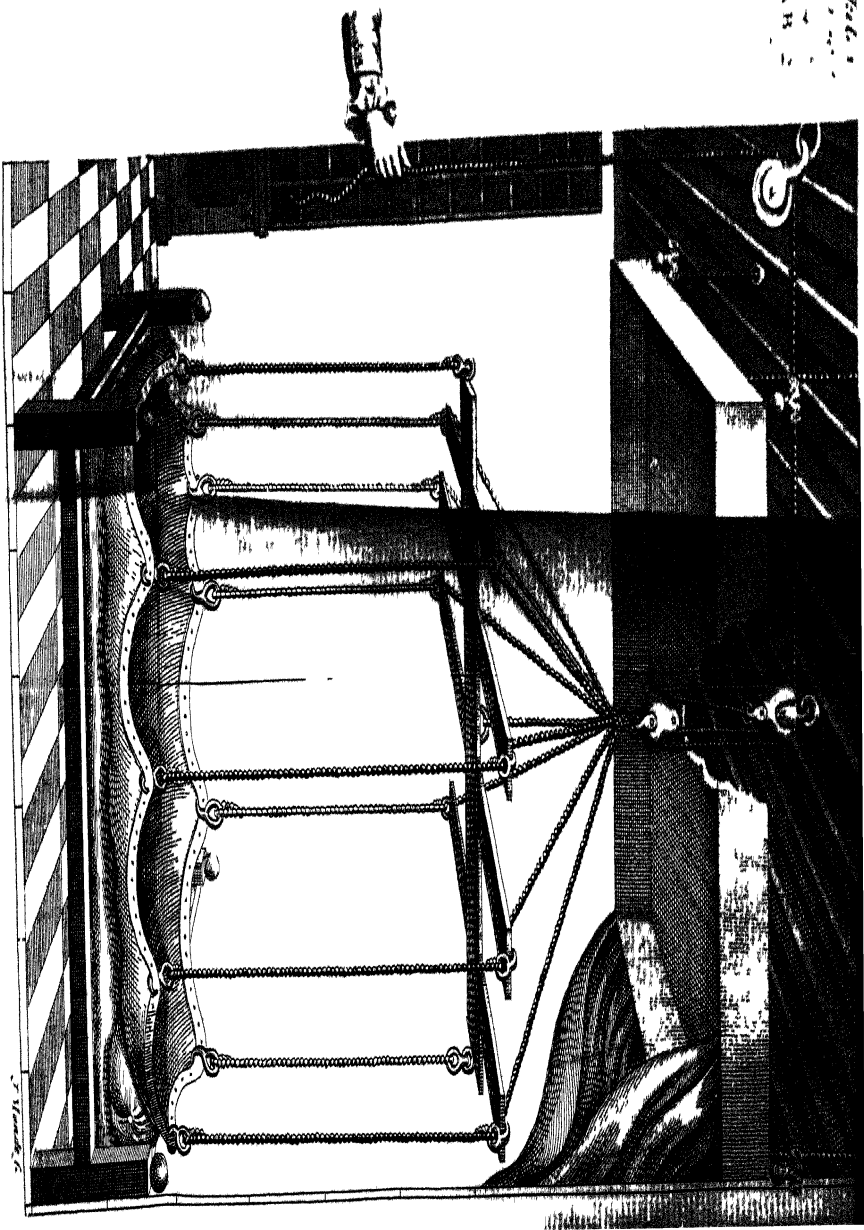
Fig. I. p. 301.



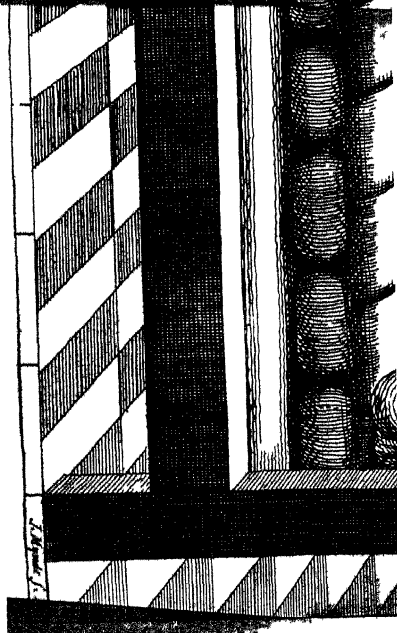
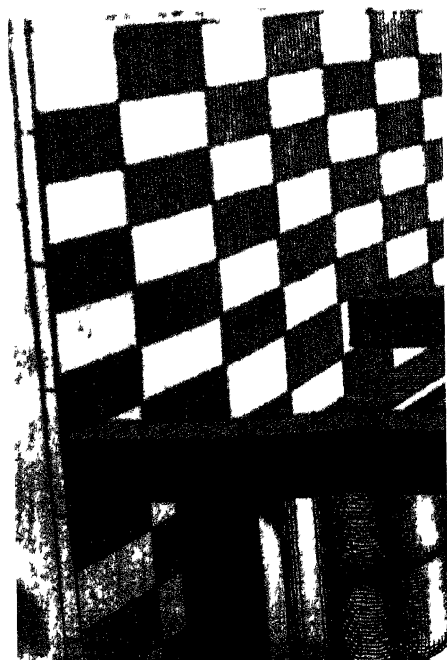
*Fig. II.
p. 363.*

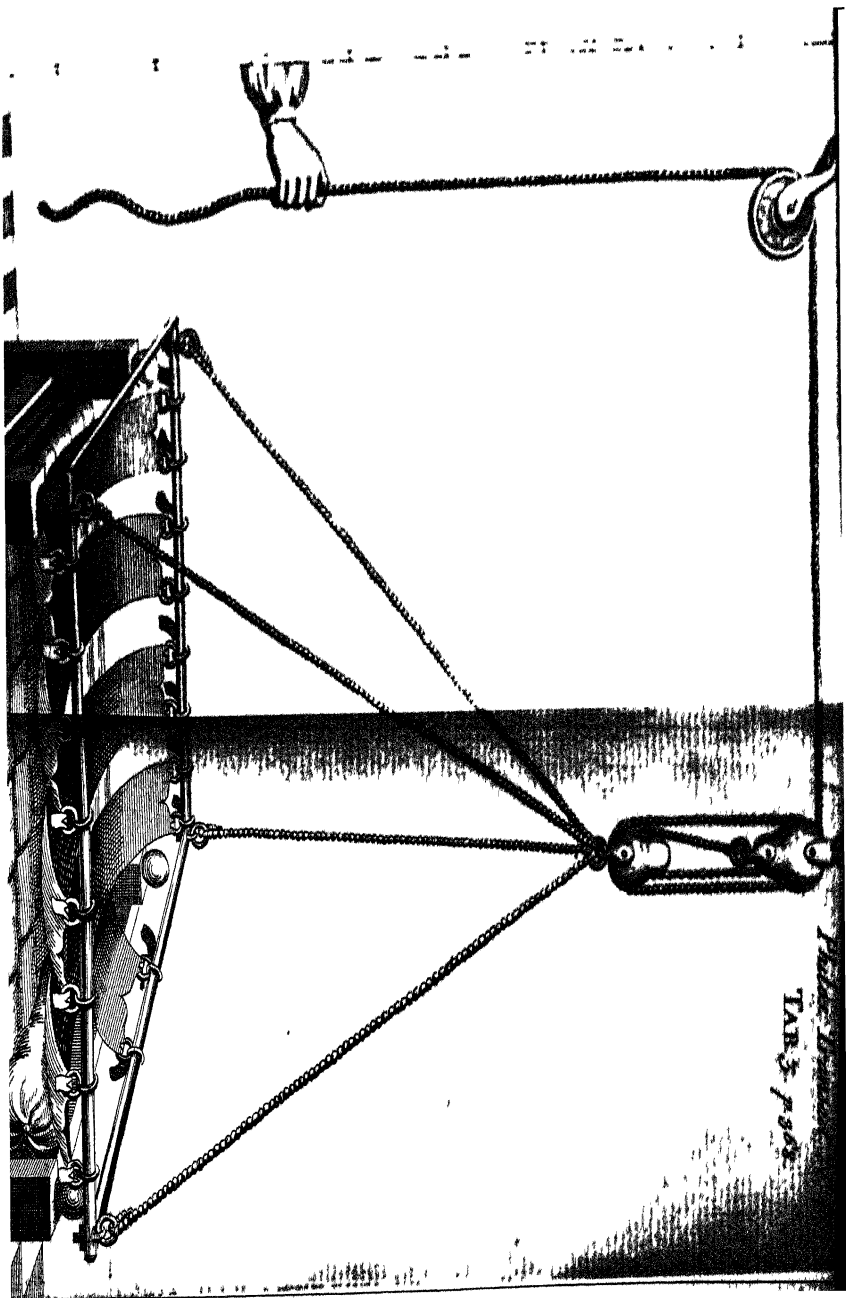


Plaque
1789
1790
1791
1792
1793
1794
1795
1796
1797
1798
1799
1800



27. August 18. 17





The CONTENTS.

- V. *Excerpta ex Epistola Cl. Viri Joh. Ambrosii Beureri ad Petr. Collinson, R. S. S. de Natura Succini.* 322.
- VI. *An Account of a Book intituled, A Treatise of Fluxions, in Two Books, by Colin Mc Laurin, A. M. Professor of Mathematics in the University of Edinburgh, and Fellow of the ROYAL SOCIETY, 4^{to}. in Two Volumes, Pages 763.* 325.
- VII. *De Calculo prægrandi a Muliere cum Urina excreto Observatio D^m Antonii Leprotti, R. S. S. Pont. Max. Archiat. per Abbatem Didacum de Re-villas, R. S. S. ad D. Smart Lethicullier R. S. S. transmissa.* 363.
- VIII. *The Description of a Machine for dressing and curing Patients, who are very unwieldy, and are under the Surgeon's Hands for some Ailment on the Back, the Os Sacrum, &c. or are apprehensive of it. By M. le Cat, F. R. S. Surgeon to the Hotel Dieu at Rouan, and Royal Demonstrator in Anatomy and Surgery: Abstracted from the French by P. H. Z. F. R. S.* 364.
- IX. *An Account of a Treatise, (intituled, D. Alberti Halleri, Archiatri Regii & Elect. Medicin. Anatomie Botan. Prælect. &c. Enumeratio Methodica Stirpium Helvetiæ indigenarum. Qua omnium brevis Descriptio & Synonymia, Compendium Virium Medicarum, dubiarum Declaratio, novarum & rariorum uberior Historia & Icones continentur. Gottingiæ, 1742. in Folio) extracted and translated from the Latin by William Watson, F. R. S.* 369.

ERRATUM.

N^o 467. p. 1. for Jan. 21. read Jan. 20.

I. *Johannis Marchionis Poleni, R. S. S. De novis quibusdam Cogitationibus ad explorandum, num Pendula vi aliqua centrifuga perturbentur, Commentariolum Illustrissimæ SOCIETATI REGALI Londinensi oblatum.*

Presented at a Meeting of the Royal Society, on January 20. 1742-3.

UTI pro re parva exordio, quod adhibuerit olim vir summus pro maximis rebus, quid verat? cum facturuse sim, operæ pretium, si propositionem quandam meam ad Pendulorum motus pertinentem, perscripserim, nec satis sciam; nec, si modo aliquo sciam, dicere ausim. Utcunque crit, tentare juvabit.

II. Res autem, de qua acturus sum, est vis illa Centrifuga, ex cujus incremento, arguunt bene multi viri doctissimi, vim gravitatis imminui, & hujusmodi imminutionem ex retardatione motus Pendulorum prope Æquatorem cognosci facile posse, constituunt.

III. Non tamen earundem retardationum observationes *D. Richeri*, in Insula *Cayena* habitas, præclaras easdemque late pervulgatas hic recensebo; non illas, quas viri celeberrimi *Edm. Halleius*, *D. Varin*, *D. Desbajes*, *D. du Glos*, *Jo. Matthæus de Chazelles*, *P. Ludovicus Feuillée*, *Claud. Ant. de Couplet* instituisse, atque literis mandaverunt: has habebō tanquam notas, ut quas docti quique passim probe norint.

IV. Cum autem ad investigandam retardationum illarum causam nonnulli vias alias (diversas ab Centrifuga vi) sint ingressi, & eam, gratia exempli, in diductione virgæ ferreæ Penduli se invenisse, quidam opinati sint; non ego de hujusmodi explicationibus hoc loco verba faciam; sed habebo ceu ratum hoc tempore, eas retardationes proficisci præsertim ab imminuta vi gravitatis.

V. Imminutio autem illa vis gravitatis quanta initis calculis sit computanda, nec quærere, nec commemorare, ad præsens meum pertinet institutum.

VI. Neque fuscè versabor in exponendo quodam veluti elemento, cujus usum in illiusmodi calculis subducendis prætermittendum non esse, mihi videtur. Si enim Horologia, vi appensorum *Ponderum* incitata, ad explorandas variis in Regionibus differentias velocitatum Pendulorum ex varia vi gravitatis oriundas, adhibeantur: nonne rationi consonum est, co casu, in supputationibus locum suum obtinere etiam considerationem mutationum gravitatis ipsorum *Ponderum*; nempe mechanici illius principii, a quo Horologiorum motus progignitur, conservaturque? Non me latet, quàm variè, variatis *Ponderibus*, motus in Horologiis aut accelerentur, aut retardentur: & quàm exiguæ differentię motuum respondeant prægrandibus *Ponderum* differentiis (docte hæc & egregie in *Commentariis Regiæ Scientiarum Academiæ* An. 1720. pag. 208. sunt demonstrata). Animadvertendum tamen est, magnum *Newtonum* in hujusmodi calculo (in *Philosophiæ Naturalis Principiis*, Edit. 1726. pag. 421.) non neglexisse minimam rem, minimumve discrimen, hoc est, sextam partem lineæ unitus. Itaque vel hujusmodi rationem mutationis gravitatis in *Ponderibus*

deribus non prorsus prætereundam duxi: quando non una experientia indicavit, quosdam effectus ex tenuissimis causis melius, quam ex validioribus percipi posse. Sed de hac re vel satis; vel, quia extra propositum meum, plus satis dictum est.

VII. Nunc ad rem, quam proponere mens est, propius accedam. Quoties instituitur quæstio de exploranda cognoscendaque Centrifuga vi, toties solent, cum in finem, Observationes habitæ in regionibus, immani intervallo distitis, inter se comparari. At cogitare ego cœpi, num ad eundem finem posset aliquid obtineri, quamvis inter Observationes instituendas, nulla regionis, nulla loci mutatio intercedat. Ut vero cogitata mea proferre facilius possim, juvabit ordiri ab iis, quæ doctissimus *Christianus Hugenius* in sua de *Causa Gravitatis Dissertatione* proposuit, cum in eo esset, *ut detegeret, quota parte minui debeat Pendulum, quod e Gallia sub lineam æquinoctialem fertur*. Verba ejus mox describam: sed, quod attinet ad Fig. I. in TAB. præfixâ; cum *Hugenii* Figura eam referat speciem, ut omnes lineæ in uno eodemque plano esse videantur; tentavi, si possem novum schema formare, quod *Armillaire Sphære* partem scenographica ratione (itaque tenues lineas adhibere non licuit) quodammodo repræsentaret; & quo plus juvaretur contemplantium phantasia, & eidem aptius addi possent partes illæ, quæ ad propositum nostrum explicandum recte conducere.

VIII. *Circulus* (*Hugenii* verba sunt) *PAQE* repræsentat terram sectam à plano transeunte per utrumque Polum P, Q (itaque *Circulus* ille erit Meridianus). Centrum est C: *Circulus Equinoctialis* EFAG: *parallelus Parisiensis* DNO: *Parisi in*

D: KH repræsentat funem sustentem massam plumbi H, quod recedit a perpendiculari KDC, quia rejicitur, per motum circulearem, secundum lineam DM, quam pono transire per pondus H. Est autem ea DM linea tangens Circulum DNO, parallelum Parisiensem, in puncto D.

IX. Nunc si lubeat scire qui debeat esse situs fili KH, & quanto minus plumbum H sic gravitet, quàm si penderet perpendiculariter secundum KD, considerare oportet punctum H ac si trahatur a tribus filis HC, HM, HK; e quibus HC centrum Terræ versus trahit toto pondere, quod haberet plumbum, si Terra immota staret: HM trahit juxta propriam directionem, cum ea vi (Centrifuga) quam dat motus Terræ in circulo DNO: & HK trahitur aut trahit cum ea vi, quæ quæritur. Producta igitur CH, & ducta KL parallela DM, notum est, tria latera trianguli HLK proportionalia esse potentiis, quæ trahunt punctum H; & latus LH respondere ei, quæ trahit per HC; latus KL ei, quæ per HM; ac latus HK ei, quæ trahit aut sustinet plumbum per filum KH. Sed triangulum KDH censetur habere latera sua æqualia lateribus trianguli HLK; quia CHL est quasi parallela CDK. Ergo latera trianguli KDH respondent iisdem potentiis: scilicet latus KD gravitati absolutæ ponderis H, quam haberet, si Terra staret immobilis; DH potentiæ quam illi tribuit motus (producens vim Centrifugam per tangentem DM) diurnus; & KH gravitati quæ quæritur. At ego vis Centrifugæ potentiam specto; cam nimirum, quæ tangenti lineæ DH respondet.

X. Hactenus itaque *Hugenii* methodo adhibita, egregia illa quidem, tantoque viro plane digna, posui
ca,

ea, quæ magnopere ad propositum meum referuntur; eatenus tamen, quatenus *considerare oportet* plumbum H *ac si trahatur a tribus filis* H C, H M, H K; eo scilicet in casu, quo a tribus his filis, sive a tribus his *potentiis* plumbum H immobile detinetur. Quod si moveri illud debeat; hoc est, si Pendulum oscillet; ego quidem suspicor in eum oscillationis motum novas considerationes esse intendendas: igitur ad hæc gradum faciam, agamque modo de partibus, quas addere Figuræ oportuit.

XI. De quibus tamen antequam dico; notabo, me ad hæc principio usum esse Figura ex solidis fili ferrei crassioris partibus affabre formata: in huiusmodi enim Figura res tota concipitur, cerniturque clarius. Tum animadvertam (considerata hypothesi Terræ motæ) in una Penduli oscillatione non describi ab ejus centro perfecte unum eundemque arcum in plano eodem: nihilo tamen secius, cum nascentes inde differentię rem meam non turbent, negligi a me tuto possunt; sufficitque hæc semel indicavisse.

XII. Acturus itaque de Figura, quam exhibeo, in ea diligenter mente concipi velim, per punctum H ductum esse planum parallelum Meridiano P A Q E, & in hoc plano signatum esse arcum B T V; qui, ita oscillante Pendulo K H ut plumbi centrum H ab eo plano numquam exiret, describeretur ab eodem centro in eo plano. Hic arcus B T V dicatur primus arcus.

XIII. Tum imaginatione percipiatur, per tangentem D M & per radium D C extendi aliud planum, & in plano hoc arcum R I S esse delineatum; qui, ita oscillante Pendulo K H, ut plumbi centrum H ab hoc plano numquam exiret, describeretur ab eodem centro,

centro in hoc plano. Manifestum autem est, duos illos arcus $BT\dot{V}$, RIS , sese in H ad rectos angulos interfecare.

XIV. His modo ita declaratis, duo peculiari attentione digni occurrunt casus; sive duæ Pendulorum oscillantium directiones sunt præsertim considerandæ: una per primum arcum $BT\dot{V}$; altera per secundum arcum RIS .

XV. Quod attinet ad primam; cum Pendulum, per primum arcum $BT\dot{V}$ oscillans, moveatur in plano, quod ab plano Meridiani $PAQE$ semper æquidistat intervallo longitudinis lineæ DH , sive tantum semper distat, quantum valet integra vis Centrifuga per tangentem DH ; perspicuum esse videtur, hoc in casu *potentiam* vis Centrifugæ per DH , *potentiam* gravitatis per HC , *potentiamque* fili secundum HK , semper inter se, etiamsi Pendulum oscillet, attemperari eadem illa ratione, quam explicavit *Hu-
genius*, & quam etiam immobili detinendo Pendulo inservire supra monuimus.

XVI. Quod ad alteram attinet: qua Pendulum movetur per secundum arcum RIS in eodem plano, in quo est Centrifugæ vis linea directionis DM . In hoc quidem casu non videtur ea vis ita agere, ut conetur distrahere plumbi centrum H ab hoc suo plano; sed dum Pendulum tendit ab R ad S , videtur etiam ipsa (quandoquidem agit in plano eodem directione sua, ab D ad M) conspirare ad augendum Penduli motum. Contra vero, dum Pendulum regreditur ab S ad R , videtur eadem Vis, directione illa sua, ab D ad M , motum Penduli retardare.

XVII. Motus ergo Penduli proprius, hoc est, qui uni gravitati centrali, agenti secundum DC , esset referendus, in primo casu excursionis per arcum
 $BT\dot{V}$,

BTV, ab vi Centrifuga variatur, quoniam afficitur a motu per DH ex vi illa Centrifuga oriundo, cum quo componatur necesse est. In secundo autem casu excursionis per arcum RIS variatur ille idem motus Penduli Proprius, quia in uno integro excursu versus plagam S, acceleratur ab eadem illa vi, directâ ab D ad H; at ab eadem vi retardatur in contrario versus oppositam plagam R recurſu.

XVIII. Itaque cum rationi & calculis consonum videatur, ab variatione, quæ fit in arcu RIS, non æquari variationem, quæ fit in arcu BTV; credibile etiam fit differentiam aliquam interesse oportere inter duos illos casus; nimirum inter motus Penduli oscillantis per secundum illum arcum RIS, & motus ejusdem Penduli oscillantis per primum arcum BTV.

XIX. Paucis vero his propositis, indicatum satis jam est id, quod ego, animadversione notationeque illius differentię, visus mihi sum invenisse. Visus nempe mihi sum invenisse modum ad aliquid de Centrifuga vi, quæ Telluris circa axem suum rotationi attribuitur, explorandum ope Observationum; quamvis inter Observationes instituendas nulla regionis, nulla loci mutatio intercedat.

XX. Casui autem primi arcus respondebit Pendulum super aliqua Meridiana linea ita collocatum, ut secundum eam lineam oscillationes quamproxime fiant: & casui arcus secundi accommodabitur Pendulum, si ita ponetur, ut linea oscillationum ad rectos angulos cum Meridiana linea constituatur. Quid si Horologiis (ad hujusmodi experimenta) aptarentur longiora Pendula; puta, longitudinis Horariorum pedum novem?

XXI. Plura non addam. Cur enim animadvertem agi de temporum pertenui hercle perque exili dif-

differentia, aut ad calculos rationesque revocanda, aut experimentis (ubi fieri iteratis tentaminibus possit) perquirenda? Si id per se satis dilucide atque manifeste jam apparet. Quod si in subtili cogitatione hac aliquid humani mihi contigerit, veritatem lubens agnoscam. Gaudet quoque vehementer, si cuipiam occasionem dederō præstandi utilia, ut veritas, quæ in occulto latere amat, perspicue detegatur.

II. *Observationes Astronomicæ habitæ in Collegio Pekinenfi a Patribus Societatis JESU, a Mense Novembri 1740. a D^o Jacobo Hodgson, R. S. S. cum REGIA SOCIETATE communicatæ.*

Presented Jan.

20. 1742-3.

1740.

h ' "

Nov. die 4. 5 55 15 *a. m.* plena immersio Satellitis primi in umbra Ψ , visa *telescop.* 13. *ped.*
 8. 12 16 5 *p. m.* plena immer. 2^{di} Satellitis Ψ , visa *eodem.*
 18 34 *p. m.* ♀ præcedens Stellam η in π occidentalior erat in asc. rect. 2' 54'' temporis, & borealior in declinatione 6' 30''.
 9. 11 2 15 *p. m.* ♂ emerfit e Luna in recta per Menclauum & Keplerum: immersio videri non potuit ob nubes in horizonte ortivo.

Nov.

1740. h. 1 11
Nov. die 9. 18 34

♀ Sequens hesternam
stellam, orientalis erat
in ascensione recta 1'.
temporis, & australior in
decl. 15' 40''. distans
21' 53''.

12. 14 16 52 p.m. plena immersio Satel-
litis 1^{mi}. *Telesf.* 13 ped.

15. 14 49 24 p.m. plena immersio 2^{di} Sa-
tellitis. *Eodem Telesf.*

16. 9 55 p.m. Satelles 3^{tius} immerfurus
in umbram disparuit in
nebula, ob quam nec
hujusemersio, nec 4^{ti} im-
mersio spectari potuit.

19. 16 9 10 p.m. plena immersio
Satell. 1^{mi}.

21. 10 37 33 p.m. plena immersio *Telesf.*
Ejusdem. 13 ped.

22. 17 21 48 p.m. plena immersio
2^{di} Satellitis.

18 43

♀ Stellam ♄ in ♍ Se-
quebatur in asc. rect. 4'
27'' temp. & erat au-
stralior in declin. 13'
20''.

26. 7 45 vesp. Stella τ in ♍ stabat in
Linea Dichotomiæ ς,
a cuspide australi in de-
clinatione australior
13' 0''.

1740.

1740. h ' ''
 Nov. die 26. 18 1 25 p.m. plena immersio }
 1 mi Satellitis. } Tel. 13
 30. 17 47 40 p.m. plena immersio }
 Satellitis 3ⁱⁱⁱ. } ped.

Dec. die 2. 17 20 *p.m.* Stella ϵ in γ supra erat & in eadem asc. rect. cum centro Platonis, borealior in declin. $12' 20''$. Plato distabat a limbo bor. & $4' 0''$.

3. 9 10 57 p.m. plena immerfio Satell.
2^{di}. *Tubo 13 pedum.*

4. 12 26 *p.m.* α obtexit Stellam η in
π, quæ immerfit contra
Byrgium; emersio ob-
nebulam non fuit ob-
servata.

IO. II 42 32 *p.m.* plena immersio
2di Satellitis.

14. 10 38 20 *p.m.* plena immersio
1 mi Satellitis. *Tel. 10*

17. 14 15 20 *p.m.* plena immersio *ped.*
2^{di} Satellitis.

19. 18 3 *p.m.* immersio Satel-
litis 1^{mi}.

1741.

Jan. die 1. 7 mane ♀ distans a Stella ν in
m $\pm 34'$. Sequebatur eam
in asc. rect. $1' 50''$,
temp. borealior in de-
clin. $19' 0''$.

Jan.

1741.

Jan. die 1. h ' " 6 p. m. Emerſio Satellitis 1mi.
Teſcop. 10 pedum.

5 59 30 — Limbus occiduus & ad
filum horarium in te-
leſcopio.

6 0 24 — ¶ ad diem filum, diſtans
a limbo bor. & 13' 0''.

1 15 — Limbus orientalis & ad
idem filum.

6 5 36 — Limbus occiduus & rur-
ſus ad idem filum ho-
rarium.

6 18 — ¶ ad idem filum, diſtans
a limbo boreo, 12' 30''.

7 55 — Limbus orient. & ad
idem filum.

11 34 — ¶ culminavit altus 73°
26'.

43 — & culminavit alt. centr.
73° 15'.

Die 2. horis matutinis partialis & Eclipſis,

h ' " 6 4 18 mane umbram iam tenuiter
ſtrinxiffe & marginem,
tubo 3 $\frac{1}{2}$ ped. notatum
eſt.

6 5 40 Initium certum inter Vie-
tam & Schikardum
tubo 6 ped. ſignatum
fuit.

10 0 Umbra ad Schikardum.

14 0 ad mare humorum.

R r 2

Jan.

1741.
Jan. die 2.

h	'	"	
6	16	0	Umbra ad Grimaldum.
	16	10	ad Capuanum.
	17	20	incipit Gassendus immergi. Centrum Grimaldi in umbra.
	18	30	totus Grimaldusmersus.
	19	0	Umbra ad Campanum.
	19	30	ad Herigonium.
	22	30	ad Tychonem.
	23	10	ad Bullialdum.
	24	0	Tycho mersus.
	24	20	Umbra ad Pitatum..
	32	0	ad Galileum.
	42	0	ad Kepplerum.
	43	0	ad Reinholdum.
	55	0	Fracastor. mergitur, umbra ad Copernicum.
7	2	0	Copernicus totus videtur esse in umbra.
	6	0	Umbra ad Wendelinum.
	10	20	Centrum ☿ in umbra.
	14	0	☿ occultatur post montes ad occasum ante medium eclipsi- sis.

Cæterum Penumbra erat exigua, & umbra bene nigra ac distincta: potuitque coelo admodum serene & clare eclipsis sat commodè spectari usque ad occasum ☿. Diameter Lunæ ante eclipsim erat in micrometro 30' 20'' circ. in ipso vero occasu tantum 30' 0''. Centrum apparentis disci erat ab sinu æstuum occid. modice versus Hipparchum.

Jan.

1741.

h ' "

- Jan. die 6. 12 52 25. *p. m.* Emerfio Satellitis 1^{mi}.
Telescop. 18 *ped.*
8. 7 20 30 *p. m.* Emerfio ejufdem. *Eodem*
dubiè.
11. 13 51 22 *p. m.* Emerfio Satellitis 2^{di}.
Telescop. 10 *ped.*
14. 2 42 40 *a. m.* Emerfio Satellitis 1^{mi}.
Telescop. 18 *ped.*
15. 9 12 36 *p. m.* Emerfio ejufdem. *Te-*
lescop. 13 *ped.*
18. 16 26 15 *p. m.* Emerfio Satellitis 2^{di}.
Telesc. 10. *ped.*
21. 5 30 *p. m.* ♂ præcedebat hefternam
 Stellam c in Π 1' 8".
 temp. in asc. rect. au-
 stralior erat in declin.
 5' 0"
22. 5 15 *p. m.* ♂ præcedens hefternam
 Stellam 2' 45". temp.
 in asc. rect. australior
 in declin. 2' 0".
- 9 41 57 *p. m.* Emerfit Satel-
 les 2^{dus}. } *Telesc.*
11. 3 19 *p. m.* Emerfit Satel-
 les 1^{us}. } 18 *ped.*
24. 5 33 24 *p. m.* Emerfio Satellitis 1^{mi}.
Telescop. 10 *ped.*
28. 7 18 *vesperi* & a margine α distabat
 19' 30".
29. 8 18 16 *p. m.* Emerfio Satellitis 2^{di}.
Teles. 13 *ped.*

Jan.

1741.

h ' "

Jan. die 29. 12 57 10 *p.m.* Emerfio Sa-
tellitis 1^{mi}.

Feb. die 3. 8 18 *p.m.* Emerfio Sa-
tellitis 3^{tii}.

5. 10 53 20 *p.m.* Emerfio Sa- } *Telesc.*
tellitis 2^{di}. 8 *ped.*

6. 2 53 *a.m.* Emerfio Sa-
tellitis 1^{mi}.

7. 9 20 35 *p.m.* Emerfio e-
jufdem.

8. 10 43 *p.m.* plena immer.

Satellititis 4^{ti}.

14 6 30

Cæpit emer-
gere idem Sa-
telles 4^{tus}.

} *Telesc.*
13 *ped.*

10. 9 16 30 *p.m.* plena immerfio Satel-
lis 3^{tii}. *Eodem.*

12. 13 32 *p.m.* Emerfit Satelles 2^{dus} vifus
Telescop. 8 *ped.*

14. 11 14 15 *p.m.* Emerfio Satellitis 1^{mi}.
Telescop. 18 *ped.*

16. 5 43 45 *p.m.* Emerfio ejufdem. *Eo-*
dem.

22. 11 44 26 *p.m.* Luna obtexit ftellam „
in 8 ftantem in recta
cum Manilio & Cenfo-
rino. Emerfio fpectari
non potuit.

23. 7 39 29 *p.m.* Emerfit Satelles 1^{us}.
Tubo 18 *ped.*

Feb. -

1741. h ' "
Feb. die 24. 9 27 p.m. Stella η in Π infra ϵ am
stabat in recta cum Ty-
chone & Platone, ab
illo in austrum distans
11' 20"
- 13 38 45 p.m. Stella μ in Π obtecta
fuit a ϵ in recta per
Tychonem & Posido-
nium; quæ non emer-
sit ante hor. 13. 55',
cum ϵ occidit post
tectum.
25. 8 26 30 p.m. Emerfio Satellitis 4^{ti}.
Telescop. 13.
- Mart. die 2. 9 36 11 p.m. Emerfio Satellitis 1^{mi}.
Telescop. 18.
11. 6 2 45 p.m. Emerfio ejusdem. Eod.
April. die 3. 6 26 35 p.m. Emerfio ejusdem. Eod.
8 14 Emerfio Satellitis 2^{di}.
Tubo. 8 ped.
10. 8 20 37 p.m. Emerfit Satelles 1^{us}. visus
Telescop. 13.
20. 10 50 42 p.m. ϵ obtexit Satellitem
3^{tium}, qui erat ad occi-
dentem χ ^{is}.
- 10 56 45 ϵ attigit limbum χ
h. 10 57' 35" fuit plena
hujus immerfio plane in
medio inter utramque
cuspitem ϵ recta versus
centrum. Alii Satel-
lites ob atmosphæram
non.

non erant bene discernibiles. Et Luna paulo post se abscondit post tecta.

1741.		h	'	"	
Maii die	3.	8	40	6	p.m. Emerfio Satellitis 1 ^{mi} . <i>Telescop.</i> 13 ped.
		9	36		Emerfio Satellitis 4 ^{ti} . <i>Eodem.</i>
Sept. die	8.	4	41	48	a.m. Immerfio Satellitis 1 ^{mi} . <i>Eodem.</i>
	24.	8	7	15	p.m. ☾ obtexit Stellam quadranguli ante caudam Ceti Australem præcedentem, quæ immerfit modice ad ortum Clcofrati.
		☾	0	13	Eadem emerfit proxime Berosum.
Oct. die	1.	5	0	8	a.m. Immerfio Satellitis 1 ^{mi} . <i>Tubo</i> 13 ped.
	15.	17	8	49	p.m. Immerfio 2 ^{di} . Satellitis cœlo nonnihil nubilo. <i>Telescop.</i> 13 ped.

III. *Extract* by John van Rixtel, F. R. S. of Mr. W. Kerſſeboom's * *Second and Third Treatiſe* — *Confirming the Manner how to know the probable Quantity of People in the Provinces of Holland and Weſt-Frieſland, beſides a Foundation on which to prove the probable Lives of Widows, and likewise a Rule whereby to know the Duration of Marriages.*

Second Part.

Read Jan. 27.
1742-3.

MR. Kerſſeboom having advanced in his *First Treatiſe*, printed Anno 1738, that the Provinces of *Holland* and *Weſt-Frieſland* contained 980,000 Souls, of all Ages, on a well-grounded Suppoſition, that annually are born in the ſaid Two Provinces 28000 Children alive; but it having been the Opinion, that this ſhould be more clearly demonſtrated, has thought it neceſſary to comply therewith. In order to which, the Author has divided the Provinces into Three general Diviſions, diſtinguiſhed with the Letters *A, B, C*; and given the Names of the ſeveral Cities, Towns, and Villages, belonging to the ſeveral Letters juſt now mentioned; and ſuppoſes, on good Grounds, (though not on a Mathematical Enumeration, which the Au-

* See an Account of the *First Part*, *Philofophical Transactions*, N^o. 450. p. 401.

thor could not do, for Reasons assigned in his First Treatise, Page 38.) that in the First Division marked

A.	— are born alive annually	3890 Children.
B.	————— annually	19070 and
C.	————— annually	5040.

which is together — annually 28000 Children.

And, as it has been proved in his First Treatise, by what has been there observed, in relation to Annuities for Life; that for every Child that is born, the whole Number of People is 35 times as many; so it will prove, that these Numbers being multiplied together, it renders 980,000 Souls.

But as it was impossible for the Author (as has been hinted before) to get an exact Account, from all Places, of the Births, Weddings, and Burials, (from which Two last the First is to be cited and proved) he proceeds to give you the chief Observations he was able to obtain; and believes that these, joined with those contained in his First Treatise, will be a sufficient Proof to his general Calculations.

Mr. *Kerffeboom* then goes on, with giving an Account how many People were buried in the City of *Dort* every Year, from 1700 to 1739 inclusive, amounting, in 40 Years, to 28977 Persons, which is annually, on an Average, 724. — The Marriages are 202 Couple annually, during the same time, which should produce (according to the Author's Calculations in his First Treatise, Page 24.) 325 Children *per* 100 Marriages, and consequently 656 Children *per Annum*; but has found it, on an Average, to be 651. — This City being a Sea-Port, and driving a large Trade to *Scotland*, and on the *Rhine*, and consequently

frequently many of the People, whose Traffick brings them to *Dort*, may die there, it is supposed, that about 680 Children are born annually there, and that consequently this City may contain 24000 Souls.

Next to this, the Author gives an Account of *Haerlem*, how many People died there in 84 Years, from 1656 to 1739 inclusive, namely, 132132 Persons, which is annually, on an Average, 1573.—The next is, how many Marriages from *Anno* 1690 to 1739 inclusive, namely, 21910, is annually 438, on an Average.—About the Births, Mr. *Kerffboom* refers to his First Treatise, Page 54. where he supposes, that 1450 Children may be born alive annually; and endeavours to demonstrate it further, by giving an Account of the Births for 60 Years, namely, from 1680 to 1739, and finds it to be 1453; from which it is calculated, that this City contains 50500 Souls, as mentioned in his First Treatise.

The next Account is that of the Burials of *Delft* and *Delftshaven*, from the Year 1724 to 1739, being 15 Years, and is found to be annually, on an Average, 723 Persons; but there is subjoined, for the greater Certainty, an Account from the Year 1696 to 1739, which proves it to be 748 Persons annually.

The Marriages are in the same Time of 44 Years, on an Average, 224 *per Annum*, which should produce 728 Children, according to the Rule laid down before, namely, 100 Marriages producing 325 Children; but is found to produce from 1690 to 1739 inclusive, to be 648 *per Annum*, on an Average; from whence it is supposed those Two Places contain 25000 Souls.—

The City of *Leyden* comes next in Consideration. It appears by a List for 50 Years, namely, from the Year 1690 to 1739 inclusive, that there have been buried in that City annually, on an Average, 1919 Persons; and married during the same Time, annually, on an Average, 558 Couple, which, agreeable to the former Rule, would produce 1813 Children *per Annum*, but is found to have been 1834 *per Annum*, on a Medium, as aforesaid; the Author concludes consequently, that this City contains 63000 Souls.

The next City in View is *Amsterdam*: It appears by a List, that since the Year 1696 to 1738 inclusive, there have been buried in this City 7323 Persons annually (*Jews* excepted); and there having been married, during the same Time of 43 Years, 2311 Couple annually, produced, according to the Author's Computation, 7134 Children annually, at a Medium; and takes it thence for certain, that *Amsterdam* contains (including 20000 *Jews*, as observed in his First Treatise, Page 21.) 241000 Souls.—The Author proceeds, in the like curious Manner, about other Places; but lest, dwelling too long on this Particular, it might prove tedious in this Place, I will proceed with observing, that the Author gives next—a Table how long 432 Widows lived during a Century, and shews it to have been near 14 Years each on a Medium; and then subjoins a List how many Years married People of different Ages continue to live probably together, before the Bonds of Matrimony, by the Death of either Party, are dissolved; namely,

		live between
Those whose Ages together are	40	24 and 25 Years.
- - - - -	50	22 and 23.
- - - - -	60	23 and 21.
- - - - -	70	19 and 20.
- - - - -	80	17 and 18.
- - - - -	90	14 and 15.
- - - - -	100	12 and 13.

And finishes with rejecting the Method of calculating the Quantity of People after the manner of *Vossius*, *Auzout*, *Petty*, and others.—

Third Part.

The *Third* Treatise contains, 1st, A Copy of a Letter wrote by the Author in the Beginning of the Year 1741, to the worthy Mr. *John Eames*, one of the Fellows of this Honourable SOCIETY, and laid before the same soon after, by the said Gentleman.

2^{dly}, A Demonstration, in 29 Tables, that Mr. *Simpson's* Calculation of Lives, as 1 to 26, is a Mistake, and his own Hypothesis, as 1 to 35, right; and proves from Mr. *Maitland's* Observations, Page 541. that Children in *London*, of Two Years old, continue to live, on a Medium, above 37 Years; and observes, that the Learned Dr. *Halley's* Table has it full 38 Years and a half.—

The Author supposes, 3^{dly}, That out of every 100 Children born, Five come dead into the World; and that out of every 100 Children born alive, near 20 die under a Year old; and shews, 4^{thly}, how much Mr. *Simpson* differs in his Calculation;—namely, That full 32, out of 100 Children, die, under a Year old.—

The

ance, considering Two of the Stems alone contain each above 1000 Rings: Some of these Stems were above Two Yards and an half high. I have to add, that this Plant was a Seedling last Year, transplanted out of the Fields on the End of a sloping Strawberry-bed; and I counted the Rings in the Middle of last *July*, when it had Thousands of Flowers upon it, which, with Thousands that must still succeed, might very probably produce more than 50000 Seeds * more, considering 1000 Rings contain 12000 Seeds and more; and if we multiply the Number of Rings actually counted, by 14, the Number of Seeds contained in one Ring, instead of 12, we shall have an Addition of 20000 Seeds, all which, added together, amount to 200000, the possible Increase of one Seed.

Macclesfield, Sept. 1. 1742.

Joseph Hobson.

V. Excerpta ex *Epistola Cl. Viri Joh. Ambrosii Beureri ad Petr. Collinson, R. S. S. de natura Succini.*

* *

Read Jan. 27.
1742-3.

DE Succino non solum *negative*, sed etiam *affirmative*, hæc est Sententia mea: Succinum vel Ambram citrinam succum esse arborum resinofum nego & pernego, ob sequentes ratiunculas. Primum mihi non verisimile videtur *istum* succinum per terram transire in mare: nam

* Even supposing many of the Flowers to produce no Seed.

unde

unde iste transitus? cum *tamen* arbores mari non tam *propinquæ* sint.

Deinde neutiquam hæc resina, sicut aqua, terram permeare, & in illa tam copiose se diffundere queat; sed potius, si hoc possibile esset, in superficie terræ hæreret, rigeret, & consisteret.

Præterea calor solis, quanquam magnus & continuus, neutiquam ejusmodi flumen resinarum efficere vel producere potest; quo multi subterranei tractus expleantur. *Exsudatio* enim resinarum per guttulas fieri solet, quarum minima pars terram attingit, sed potius cuti arboris adhærent. Porro cur in ejusmodi locis succinum haud raro reperitur, ex. gr. in montibus, fossis ac foveis, & quibus tamen nunquam arbores fatæ fuerunt? Postremo argumenta quæ honoratissimus amicus ex distillatione acidi vitriolici cum terebinthina eduxit, probanda non probant, quia quidem aliquid bituminosi exinde producitur, quod vero nondum succinum verum est: caret enim productum interna æquali mixtione, sed etiam *diaphanitate*, *elasticitate*, duritieque. Hoc enim faciliiori modo, & fere extempore, produci queat per mixtionem olei alicujus destillati ætherei, cum acido vitriolico *concentrato*, ex qua mixtione statim quoddam bituminosum, sed non succinum oritur.

Sed etiam nunc addo, ea qua par est animi observantia ac modestia, quid ego, secundum tenues ingenii mei vires, de origine succini sentio. Succinum originem suam non ex vegetabili, sed minerali, id est, ex tencro bituminoso (ex. gr. oleo Naphtæ) & acido sulphureo vitriolico, quod in forma halitus se immiscet, & eo ipso illud statim indurefcit, trahere mihi credibile videtur. Id quoque probat succinum

fossile, ubi semper; uti Tu ipse, VIR CLARISSIME, in literis TUIS illius rei memoriam fecisti; in ejusmodi ductibus, ex quibus succinum effoditur: in tractu nempe luti cœrulei etiam lignum bituminosum, lithanthraces, minera vitriolica (haud raro aluminis) verumque succinum permixtis inveniri solet, quod propria experientia me docuit. Succinum vero, quod in *mari* reperitur, non alio modo oritur, quam illud, quod ex ductibus montium promitur; nisi quod mare æstuans illud fluctibus suis, & ductibus vi expellat, & partim ad ripas, partim vero in abyssum maris evomat.

Filum nunc abrumpo, propter arctos limites epistolæ meæ præfixos, hoc unicum adhuc adjiciens; quod quemadmodum acidum vitriolicum una cum bituminoso formam & speciem succini producat; illud acidum etiam in forma *subtilisata & concentrata* (sine aliqua destructione partis alicujus *constituti*) hoc penitus dissolvat, & in eodem statu rursus relinquat, pristinamque illius duritiem, *diaphanitatem, & elasticitatem* reducat.

Vale, sis faustus felixque, & nunquam immemor
Tui addictissimi.

Dab. Noribergæ,
die 20 Octob. 1742.

VI. *An Account of a Book intituled, A Treatise of Fluxions, in Two Books, by Colin Mc Laurin, A. M. Professor of Mathematics in the University of Edinburgh, and Fellow of the ROYAL SOCIETY, 4^{to}. in Two Volumes, Pages 763.*

Presented Jan.
27. 1742-3.

THE Author's first Design, in composing this Treatise, was to establish the Method of Fluxions on Principles equally evident and unexceptionable with those of the antient Geometricians, by Demonstrations deduced after their Manner, in the most rigid Form, and by illustrating the more abstruse Parts of the Doctrine, to vindicate it from the Imputation of Uncertainty or Obscurity. But he has likewise comprehended in this Work the Application of Fluxions to the most important geometrical and philosophical Inquiries. It consists of an Introduction, and Two Books. In the Introduction he gives an Abstract of the Discoveries of the Antients in the higher Parts of Geometry, with Observations on their Method, and those that first succeeded to it. The First Book treats of Fluxions in a geometrical Method, and the Second treats of the Computations.

In the Introduction we have an Abstract not only of the Discoveries of the Antients in the higher Parts of Geometry, but likewise of their Demonstrations. After an Account of the Propositions of this kind, that are to be found in the Twelfth Book of *Euclid*,

there follows a Summary of what is most material in the Treatises of *Archimedes*, concerning the Sphere and Cylinder, Conoids and Spheroids, the Quadrature of the *Parabola* and the spiral Lines. The Demonstrations are not precisely in the same Form as those of *Archimedes*, but are often illustrated from the elementary Propositions concerning the Cone, or Corollaries from them, after the Example of *Pappus*, (*Coll. Math.* Prop. 21st, Lib. 4.) from whom a Proposition is demonstrated, and rendered more general, concerning the Area of the Spiral that is generated on a spherical Surface by the Composition of Two uniform Motions analogous to those by which the Spiral of *Archimedes* is described on a Plane. This Area, though a Portion of a curve Surface, is found to admit of a perfect Quadrature, and this Proposition concludes the Abstract. He takes occasion from these Theorems to demonstrate some Properties of the Conic Sections, that are not mentioned by the Writers on that Subject; and there are more of this kind described in the XIth and XIVth Chapters of the First Book.

It is known, that if a Parallelogram, circumscribed about a given Ellipse, have its Sides parallel to the conjugate Diameters, then shall its Area be of an invariable or given Magnitude, and equal to the Rectangle contained by the Axes of the Figure; but this is only a Case of a more general Proposition. For if, upon any Diameter produced without the Ellipse, you take Two Points, one on each Side of the Centre at equal Distances from it, and the Four Tangents be drawn from these Points to the Ellipse, those Tangents shall form a Parallelogram, which is
always

always of a given or invariable Magnitude, when the Ellipse is given, if the *Ratio* of those Distances to the Diameter be given; and when the *Ratio* of those Distances to the Semidiameter is that of the Diagonal of a Square to the Side, (or of $\sqrt{2}$ to 1,) the Parallelogram has its Sides parallel to conjugate Diameters. It is likewise shown here, how the Triangles, *Trapezia*, or Polygons of any kind are determined, which, circumscribed about a given Ellipse, are always of a given Magnitude.

There is also a general Theorem concerning the *Frustum* of a Sphere, Cone, Spheroid, or Conoid, terminated by parallel Planes, when compared with a Cylinder of the same Altitude on a Base equal to the middle Section of the *Frustum* made by a parallel Plane. The Difference betwixt the *Frustum* and the Cylinder is always the same in different Parts of the same, or of similar Solids, when the Inclination of the Planes to the Axis, and the Altitude of the *Frustum*, are given. This Difference vanishes in the parabolic Conoid. It is the same in all Spheres; being equal to half the 'Content' of a Sphere of a Diameter equal to the Altitude of the *Frustum*. In the Cone it is One-fourth of the Content of a similar Cone of the same Height with the *Frustum*; and in other Figures it is reduced to the Difference in the Cone.

In the Remarks on the Method of the Antients, the Author observes, that they established the higher Parts of their Geometry on the same Principles as the Elements of the Science, by Demonstrations of the same kind; that they seem to have been careful not to suppose any thing to be done, till by a previous Problem

blem they had shown how it was to be performed: Far less did they suppose any thing to be done, that cannot be conceived to be possible, as a Line or Series to be actually continued to Infinity, or a Magnitude to be diminished till it become infinitely less than it was. The Elements into which they resolved Magnitudes were always finite, and such as might be conceived to be real. Unbounded Liberties have been introduced of late, by which Geometry (wherein every thing ought to be clear) is filled with Mysteries, and Philosophy is likewise perplexed. Several Instances of this kind are mentioned. The Series 1, 2, 3, 4, 5, 6, 7, &c. is supposed by some to be actually continued to Infinity; and, after such a Supposition, we are puzzled with the Question, Whether the Number of finite Terms in such a Series is finite or infinite. In order to avoid such Suppositions, and their Consequences, the Author chose to follow the Antients in their Method of Demonstration as much as possible. Geometry has been always considered as our surest Bulwark against the Subtleties of the Sceptics, who are ready to make use of any Advantages that may be given them against it*; and it is important, not only that the Conclusions in Geometry be true, but likewise that their Evidence be unexceptionable. However, he is far from affirming, that the Method of Infinitesimals is without Foundation, and afterwards endeavours to justify a proper Application of it.

The Grounds of the Method of Fluxions are described in Chap. I. Book I. and again in Chap. I.

* See *Bayle's Dictionary*, Article *Zeno*.

Book II. In the former, Magnitudes are conceived to be generated by Motion, and the Velocity of the generating Motion is the Fluxion of the Magnitude. Lines are supposed to be generated by the Motion of Points. The Velocity of the Point that describes the Line is its Fluxion, and measures the Rate of its Increase or Decrease. Other Magnitudes may be represented by Lines that increase or decrease in the same Proportion with them; and their Fluxions will be in the same Proportion as the Fluxions of those Lines, or the Velocities of the Points that describe them. When the Motion of a Point is uniform, its Velocity is constant, and is measured by the Space which is described by it in a given Time. When the Motion varies, the Velocity at any Term of the Time is measured by the Space which would be described in a given Time, if the Motion was to be continued uniformly from that Term without any Variation. In order to determine that Space, and consequently the Velocity which is measured by it, Four Axioms are proposed concerning variable Motions, Two concerning Motions that are accelerated, and Two concerning such as are retarded. The First is, That the Space described by an accelerated Motion is greater than the Space which would have been described in the same Time, if it had not been accelerated, but had continued uniform from the Beginning of the Time. The Second is, That the Space which is described by an accelerated Motion, is less than the Space which is described in an equal Time by the Motion which is acquired by that Acceleration continued afterwards uniformly. By these, and Two similar Axioms concerning retarded Mo-

tions,

tions, the Theory of Motion is rendered applicable to this Doctrine with the greatest Evidence, without supposing Quantities infinitely little, or having recourse to prime or ultimate *Ratios*. The Author first demonstrates from them all the general Theorems concerning Motion, that are of Use in this Doctrine; as that when the Spaces described by Two variable Motions are always equal, or in a given *Ratio*, the Velocities are always equal, or in the same given *Ratio*; and conversely, when the Velocities of Two Motions are always equal to each other, or in a given *Ratio*, the Spaces described by those Motions in the same Time are always equal, or in that given *Ratio*; that when a Space is always equal to the Sum or Difference of the Spaces described by Two other Motions, the Velocity of the First Motion is always equal to the Sum or Difference of the Velocities of the other Motions; and conversely, that when a Velocity is always equal to the Sum or Difference of Two other Velocities, the Space described by the First Motion is always equal to the Sum or Difference of the Spaces described by these Two other Motions. In comparing Motions in this Doctrine, it is convenient and usual to suppose one of them uniform; and it is here demonstrated, that if the Relation of the Quantities be always determined by the same Rule or Equation, the *Ratio* of the Motions is determined in the same manner, when both are supposed variable. These Propositions are demonstrated strictly by the same Method which is carried on in the ensuing Chapters for determining the Fluxions of the Figures.

In Chap. II. a Triangle that has Two of its Sides given in Position, is supposed to be generated by an Ordinate moving parallel to itself along the Base. When the Base increases uniformly, the Triangle increases with an accelerated Motion, because its successive Increments are *Trapezia*, that continually increase. Therefore, if the Motion with which the Triangle flows, was continued uniformly from any Term for a given Time, a less Space would be described by it than the Increment of the Triangle which is actually generated in that Time by Axiom I. but a greater Space than the Increment which was actually generated in an equal Time preceding that Term, by Axiom II. and hence it is demonstrated, that the Fluxion of the Triangle is accurately measured by the Rectangle contained by the corresponding Ordinate of the Triangle, and the right Line which measures the Fluxion of the Base. The Increment which the Triangle acquires in any Time, is resolved into Two Parts; that which is generated in consequence of the Motion with which the Triangle flows at the Beginning of the Time, and that which is generated in consequence of the Acceleration of this Motion for the same Time. The latter is justly neglected in measuring that Motion (or the Fluxion of the Triangle at that Term), but may serve for measuring its Acceleration, or the Second Fluxion of the Triangle. The Motion with which the Triangle flows, is similar to that of a Body descending in free Spaces by an uniform Gravity, the Velocity of which, at any Term of the Time, is not to be measured by the Space described by the Body in a given Time, either before or after that Term, because the

Motion continually increases, but by a Mean between these Spaces.

When the Sides of a Rectangle increase or decrease with uniform Motions, it may be always considered as the Sum or Difference of a Triangle and *Trapezium*; and its Fluxion is derived from the last Proposition. If the Sides increase with uniform Motions, the Rectangle increases with an accelerated Motion; and in measuring this Motion at any Term of the Time, a Part of the Increment of the Rectangle, that is here determined, is rejected, as generated in consequence of the Acceleration of that Motion.

The Fluxions of a curvilineal Area (whether it be generated by an Ordinate moving parallel to itself, or by a Ray revolving about a given Centre) and of the Solid, generated by the Area revolving about the Base, are determined by Demonstrations of the same kind; and when the Ordinates of the Figure increase, the Increment of the Area is resolved in like manner into Two Parts, one of which is only to be retained in measuring the Fluxion of the Area, the other being rejected as generated in consequence of the Acceleration of the Motion with which the Figure flows. An Illustration of Second and Third Fluxions is given by resolving the Increment of a Pyramid or Cone into the several respective Parts that are conceived to be generated in consequence of the First, Second, and Third Fluxions of the Solid, when the Axis is supposed to flow uniformly.

In Chap. V. a Series of Lines in Geometrical Progression are represented by an easy Construction. The First Term being supposed invariable, and the Second to increase uniformly, all the subsequent
Terms

Terms increase with accelerated Motions. The Velocities of the Points that describe those Lines being compared, it is demonstrated from the Axioms by common Geometry, that the Fluxions of any Two Terms are in a *Ratio* compounded of the *Ratio* of the Terms, and of the *Ratio* of the Numbers that express how many Terms precede them in the Progression.

In the VIth Chapter, the Nature and Properties of Logarithms are described after the celebrated Inventor; and it is observed, that he made use of the very Terms *Fluxus* and *Fluat* on this Occasion. A Line is said to increase or decrease *proportionally*, when the Velocity of the Point, that describes it, is always as its Distance from a certain Term of the Line; and if in the mean time another Point describes a Line with a certain uniform Motion, the Space described by the latter Point is always the Logarithm of the Distance of the former from the given Term. Hence the Fluxion of this Distance is to the Fluxion of its Logarithm as that Distance is to an invariable Line; and the Fluxions of the Quantities that have their Logarithms in an invariable *Ratio*, are to each other in a *Ratio* compounded of this invariable *Ratio*, and of the *Ratio* of the Quantities themselves. Some Propositions are demonstrated, that relate to the Computation of Logarithms; but this Subject is prosecuted farther in the Second Book. The Logarithmic Curve is here described, with the Analogy betwixt Logarithms and Hyperbolic *Ratios*.

In the VIIth Chapter, after a general Definition of Tangents, it is demonstrated, that the Fluxions of the Base, Ordinate, and Curve, are in the same Proportion.

tion to each other, as the Sides of a Triangle respectively parallel to the Base, Ordinate, and Tangent. When the Base is supposed to flow uniformly, if the Curve be convex towards the Base, the Ordinate and Curve increase with accelerated Motions; but their Fluxions at any Term are the same as if the Point which describes the Curve had proceeded uniformly from that Term in the Tangent there. Any further Increment which the Ordinate or Curve acquires, is to be imputed to the Acceleration of the Motions with which they flow. A Ray that revolves about a given Centre, being supposed to meet any Curve and an Arc of a Circle described from the same Centre, the Fluxions of the Ray, Curve, and circular Arc, are compared together; and several other Propositions concerning Tangents are demonstrated from the Axioms. The next Chapter treats of the Fluxions of curve Surfaces in a similar manner.

The IXth Chapter treats chiefly of the greatest and least Ordinates of Figures, and of the Points of contrary Flexure and Cuspids. The Fluxion of the Base being given, when the Fluxion of the Ordinate vanishes, the Tangent becomes parallel to the Base, and the Ordinate most commonly is a *Maximum* or *Minimum*, according to the Rule given by Authors upon this Subject. But if the Second Fluxion of the Ordinate vanish at the same time, and the Third Fluxion be real, this Rule does not hold, for the Ordinate is in that Case neither a *Maximum* nor *Minimum*. If the First, Second, and Third Fluxions vanish, and the Fourth Fluxion be real, the Ordinate is a *Maximum* or *Minimum*. The general Rule demonstrated in this Chapter, and again in the last Chapter of the

Second

Second Book, is, that when the First Fluxion of the Ordinate, with its Fluxions of any subsequent successive Orders, vanish, and the Number of all these Fluxions that vanish is odd, then the Ordinate is a *Maximum* or *Minimum*, according as the Fluxion of the next Order to these is negative or positive. The Ordinate passes through a Point of contrary Flexure, when its Fluxion becomes a *Maximum* or *Minimum*, supposing the Curve to be continued on both Sides of the Ordinate. Hence the common Rule for finding the Points of contrary Flexure is corrected in a similar manner. Such a Point is not always formed when the Second Fluxion of the Ordinate vanishes; for if its Third Fluxion likewise vanishes, and its Fourth Fluxion be real, the Curve may have its Cavity turned all one Way. The same is to be said, when its Fluxions of the subsequent successive Orders vanish, if the Number of all those that vanish be even. Other Theorems are subjoined relating to this Subject.

The Xth Chapter treats of the Asymptotes of Lines, the Areas bounded by them and the Curves, the Solids generated by these Areas, of spiral Lines, and the Limits of the Sums of Progressions. The Analogy there is betwixt these Subjects, induced the Author to treat of them in one Chapter, and illustrate them by one another. He begins with Three of the most simple Instances of Figures that have Asymptotes. In the common Hyperbola, the Ordinate is reciprocally as the Base, and therefore decreases while the Base increases, but never vanishes, because the Rectangle contained by it and the Base is always a given Area, and it is assignable at any assignable Distance, how

how great soever. The Points of the Conchoid are determined by drawing right Lines from a given Centre, and upon these produced from the Asymptote, taking always a given right Line; so that the Curve never meets the Asymptote, but continually approaches to it, because of the greater and greater Obliquity of this right Line. The Third is the Logarithmic Curve, wherein the Ordinates, at equal Distances, decrease in Geometrical Proportion, but never vanish, because each Ordinate is in a given *Ratio* to the preceding Ordinate. Geometrical Magnitude is always understood to consist of Parts; and to have no Parts, or to have no Magnitude, are considered as equivalent in this Science *. There is, however, no Necessity for considering Magnitude as made up of an infinite Number of small Parts; it is sufficient, that no Quantity can be supposed to be so small, but it may be conceived to be diminished further; and it is obvious, that we are not to estimate the Number of Parts that may be conceived in a given Magnitude, by those which in particular determinate Circumstances may be actually perceived in it by Sense; since a greater Number of Parts become visible in it by varying the Circumstances in which it is perceived.

It is hardly possible to give a tolerable Extract of this or the following Chapters, without Diagrams and Computations: We shall therefore observe only, that after giving some plain and obvious Instances, wherein a Quantity is always increasing, and yet never

* See *Euclid's Elements*, Def. I. Lib. I.

amounts to a certain finite Magnitude (as, while the Tangent increases, the Arc increases, but never amounts to a Quadrant); this is applied successively to the several Subjects mentioned in the Title of the Chapter. Let the Figure be concave towards the Base, and suppose it to have an Asymptote parallel to the Base; in this Case the Ordinate always increases while the Base is produced, but never amounts to the Distance between the Asymptote and the Base. In like manner a curvilinear Area, in a Second Figure, may increase, while the Base is produced, and approach continually to a certain finite Space, but never amount to it: This is always the Case, when the Ordinate of this latter Figure is to a given right Line, as the Fluxion of the Ordinate of the former is to the Fluxion of the Base; and of this various Examples are given. A Solid may increase in the same manner, and yet never amount to a given Cube or Cylinder, when the Square of the Ordinate of the latter Figure is to a given Square, as the Fluxion of the Ordinate of the first Figure is to the Fluxion of the Base. A Spiral may in like manner approach to a Point continually, and yet in any Number of Revolutions never arrive at it; and there are Progressions of Fractions that may be continued at Pleasure, and yet the Sum of the Terms may be always less than a given Number. Various Rules are demonstrated, and illustrated by Examples, for determining when a Figure has an Asymptote parallel or oblique to the Base; when the Area terminated by the Curve and the Asymptote has a Limit which it never exceeds, or may be produced till it surpass any assignable Space; when the Solid generated by that Area,

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the Surface generated by the Perimeter of the Curve, the spiral Area generated by the revolving Ray, the spiral Line itself, or the Sum of the Terms of a Progression, have such Limits or not; and for measuring those Limits. The Author insists on these Subjects, the rather that they are commonly described in very mysterious Terms, and have been the most fertile of Paradoxes of any Parts of the higher Geometry. These Paradoxes, however, amount to no more than this: That a Line or Number may be continually acquiring Increments, and those Increments may decrease in such a manner, that the whole Line or Number shall never amount to a given Line or Number. The Necessity of admitting this is obvious enough, and is here shewn from the Nature of the most common geometrical Figures in Art. 292, 293, &c. and from any Series of Fractions that decrease continually, in Art. 354, 355, &c.

The XIth Chapter treats of the Curvature of Lines, its Variation, the Degrees of Contact of the Curve and Circle of Curvature, and of various Problems that depend on the Curvature of Lines. This Subject is treated fully, because of its extensive Usefulness, and because in this consists one of the greatest Advantages of the modern Geometry above that of the Antients. The Author on this, as former Occasions, begins by premising the necessary Definitions. Curve Lines touch each other in a Point, when the same right Line is their common Tangent at that Point; and that which has the closest Contact with the Tangent, or passes betwixt it and the other Curve through the Angle of Contact formed by them, being less inflected from the Tangent, is therefore less curve. Thus a
greater

greater Circle has a less Curvature than a lesser Circle; and since the Curvature of Circles may be varied indefinitely, by enlarging or diminishing their Diameters, they afford a Scale by which the Curvature of other Lines may be measured. As the Tangent is the right Line which touches the Arc so closely, that no other right Line can be drawn between them; so the Circle of Curvature is that which touches the Curve so closely, that no other Circle can be drawn through the Point of Contact between them. As the Curve is separated from its Tangent in consequence of its Flexure or Curvature, so it is separated from the Circle of Curvature in consequence of the Variation of its Curvature; which is greater or less, according as its Flexure from that Circle is greater or less.

The Tangent of the Figure being considered as the Base, a new Figure is imagined, whose Ordinate is a Third Proportional to the Ordinate and Base of the First. This new Figure determines the Chord of the Circle of Curvature by its Intersection with the Ordinate at the Point of Contact, and by the Tangent of the Angle in which it cuts that Circle, measures the Variation of Curvature. The less this Angle is, the closer is the Contact of the Curve and Circle of Curvature, of which there may be indefinite Degrees. When the Figure proposed is a conic Section, the new Figure is likewise a conic Section; and it is a right Line when the First Figure is a *Parabola*, and the Ordinates are parallel to the Axis; or when the First Figure is an *Hyperbola*, and the Ordinates are parallel to either Asymptote. Hence the Curvature and its Variation in a conic Section are determined by several Constructions; and, amongst other The-

orems, it is shewn, that the Variation of Curvature at any Point of a conic Section is as the Tangent of the Angle contained by the Diameter which passes through that Point, and by the Perpendicular to the Curve.

When the Ordinate at the Point of Contact is an Asymptote to the new Figure, the Curvature is less than in any Circle; and this is the Case in which it is said to be infinitely little, or the Ray of Curvature is said to be infinitely great. Of this kind is the Curvature at the Points of contrary Flexure in the Lines of the Third Order. When the new Figure passes through the Point of Contact, the Curvature is greater than in any Circle, or the Ray of Curvature vanishes; and in this Case the Curvature is said to be infinitely great. Of this kind is the Curvature at the Cuspids of the Lines of the Third Order.

As Lines which pass through the same Point have the same Tangent when the First Fluxions of the Ordinate are equal, so they have the same Curvature when the Second Fluxions of the Ordinate are likewise equal; and half the Chord of the Circle of Curvature that is intercepted between the Points wherein it intersects the Ordinate, is a Third Proportional to the right Lines that measure the Second Fluxion of the Ordinate and First Fluxion of the Curve, the Base being supposed to flow uniformly. When a Ray revolving about a given Point, and terminated by the Curve, becomes perpendicular to it, the First Fluxion of the Ray vanishes; and if its Second Fluxion vanishes at the same time, that Point must be the Centre of Curvature. The same is to be said when the angular Motion of the Ray about that Point

Point is equal to the angular Motion of the Tangent of the Curve; as the angular Motion of the *Radius* of a Circle about its Centre is always equal to the angular Motion of the Tangent of the Circle. Thus the various Properties of the Circle suggest various Theorems for determining the Centre of the Curvature.

Because Figures are often supposed to be described by the Intersections of right Lines revolving about given Poles, Three Theorems are given in Prop. 18. 26. and 35. for determining the Tangents, Asymptotes, and Curvature of such Lines, from the Description, which are illustrated by Examples. A new Property of Lines of the Third Order is subjoined to Prop. 35. The Evolution of Lines is considered in Prop. 36. The Tangents of the *Evoluta* are the Rays of Curvature of the Line which is described by its Evolution; and the Variation of Curvature in the latter is measured by the *Ratio* of the Ray of Curvature of the former to the Ray of Curvature of the latter.

Sir *Isaac Newton*, in a Treatise lately published, measures the Variation of the Curvature by the *Ratio* of the Fluxion of the Ray of Curvature to the Fluxion of the Curve; and is followed by the Author, to avoid the Perplexity which a Difference in Definitions occasions to Readers, though he hints (in Art. 386.) that this *Ratio* gives rather the Variation of the Ray of Curvature, and that it might have been proper to have measured the Variation of Curvature rather by the *Ratio* of the Fluxion of the Curvature itself to the Fluxion of the Curve; so that the Curvature being inversely as the Ray of Curvature, and

consequently its Fluxion as the Fluxion of the Ray itself directly, and the Square of the Ray inversely, its Variation would have been directly as the Measure of it, according to Sir *Isaac Newton's* Definition, and inversely as the Square of the Ray of Curvature: According to this Explication, it would have been measured by the Angle of Contact contained by the Curve and Circle of Curvature, in the same manner as the Curvature itself is measured by the Angle of Contact contained by the Curve and Tangent. The Ground of this Remark will better appear from an Example: According to Sir *Isaac Newton's* Explication, the Variation of Curvature is uniform in the Logarithmic Spiral, the Fluxion of the Ray of Curvature in this Figure being always in the same *Ratio* to the Fluxion of the Curve; and yet while the Spiral is produced, though its Curvature decreases, it never vanishes; which must appear strange to such as do not attend to the Import of his Definition.—It is easy, however, to derive one of these Measures of this Variation from the other, and because Sir *Isaac Newton's* is (generally speaking) assigned by more simple Expressions, the Author has the rather conformed to it in this Treatise, but thought it necessary to give the Caution we have mentioned.

The greatest Part of this Chapter is employed in treating of useful Problems, that have a Dependence on the Curvature of Lines. First, the Properties of the Cycloid are briefly demonstrated, with the Application of this Doctrine to the Motion of Pendulums, by shewing that when the Motion of the generating Circle along the Base is uniform, and therefore may measure the Time, the Motion of the Point that de-

describes the Cycloid, is such as would be acquired by a heavy Body descending along the cycloidal Arc, the Axis of the Figure being supposed perpendicular to the Horizon. In the next place, the Caustics, by Reflexion and Refraction, are determined. If Perpendiculars be always drawn from the radiating Point to the Tangents of the Curve, and a new Curve be supposed to be the *Locus* of the Intersections of the Perpendiculars and Tangents, then the Line, by the Evolution of which that new Curve can be described, is similar and similarly situated to the Caustic by Reflexion. The Doctrine of centripetal Forces is treated at length from Art. 416. to 493.

First, a Body is supposed to descend freely by its Gravity in a vertical Line; and because the Gravity is the Power which accelerates the Motion of the Body, it must be measured by the Fluxion of its Velocity, or the Second Fluxion of the Space described by it. When the vertical Line is supposed to move parallel to itself with an uniform Motion, the Body will descend in it in the same manner as before; and the Gravity will be still measured by the Second Fluxion of the Descent, or the Second Fluxion of the Ordinate of the Curve that is traced in this Case by the Body on an immoveable Plain, and therefore is as the Square of the Velocity (which is measured by the Fluxion of the Curve) directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely, by a Proposition mentioned above. When the Gravity acts uniformly, and in parallel Lines, the Projectile, in describing any Arc, falls below the Tangent drawn at the Beginning of the Arc, as much as if it had fallen perpendicu-
larly

larly in the Vertical ; and the Time being given, the Gravity may be measured by the Space which is the Subtense of the Angle of Contact. In other Cases, when the Gravity varies, or its Direction changes, it may be measured at any Point by the Subtense of the Angle of Contact, that would have been generated in a given Time, if the Gravity had continued to act uniformly in parallel Lines from that Term, that is, by the Subtense of the Angle of Contact in the Parabola that has its Diameter in the Direction of the Force, and has the closest Contact with the Curve ; which leads us to the same Theorem as before.

In general, let the Gravity (that results from the Composition of any Number of centripetal Forces, which are supposed to act on the Body in one Plane) be resolved into a Force parallel to the Base ; then the former shall be measured by the Second Fluxion of the Ordinate, and the latter by the Second Fluxion of the Base, the Time being supposed to flow uniformly, so that the Velocity of the Body may be measured by the Fluxion of the Curve. When the Trajectory is not in one Plane, the Force is resolved in a similar manner into Three Forces, which are measured by Three Second Fluxions analogous to them.

Whether the Body move in a Void, or in a Medium that resists its Motion ; the Gravity that results from the Composition of the centripetal Forces which act upon the Body, is always as the Square of its Velocity directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely.

When

When a Body describes any Trajectory in a Void or in a Medium, by a Force directed to One given Centre, the Velocity at any Point of the Trajectory is to the Velocity by which a Circle could be described in a Void about the same Centre, at the same Distance, by the same Gravity, in the subduplicate *Ratio* of the angular Motion of the Ray drawn always from the Body to the Centre, to the angular Motion of the Tangent of the Trajectory: And, if there be no Resistance, the Velocity in the Trajectory at any Point, is the same that would be acquired by the Body, if it was to fall from that Point through One-fourth of the Chord of the Circle of Curvature that is in the Direction of the Gravity, and the Gravity at that Point was to be continued uniformly during its Descent.

If the centripetal Force be inversely as any Power of the Distance whose Exponent is any Number m greater than Unit, there is a certain Velocity (*viz.* that which is to the Velocity in a Circle at the same Distance as $\sqrt{2}$ to $\sqrt{m-1}$) which would be just sufficient to carry off the Body upwards in a vertical Line, so as that it should continue to ascend for ever, and never return towards the Centre. If the Body be projected in any other Direction with the same Velocity, it will describe a Trajectory which is here constructed: It is a *Parabola* when $m=2$, a Logarithmic Spiral when $m=3$, an Epicycloid when $m=4$, a Circle that passes through the Centre of the Forces when $m=5$, and the *Lemniscata* when $m=7$. In general, it is constructed by drawing a Perpendicular from the Centre of the Forces to a right Line given in Position, and any other Ray to the same right Line,

then

then increasing or diminishing the Angle contained by this Ray and the Perpendicular in the given *Ratio* of 2 to the Difference between 3 and m , and increasing or diminishing the Logarithm of the Ray in the same given *Ratio*. The Trajectories described in analogous Cases by centrifugal Forces, are constructed in a similar manner. These are the Figures in which the Perpendicular, from a given Centre on the Tangent, is always as some Power of the Ray drawn from the same Centre to the Point of Contact, which are afterwards found to arise in the Resolution of the most simple Cases of Problems of various kinds.

When the Area described about the Centre of an Ellipse is given, the Subtense of the Angle of Contact, drawn through one Extremity of the Arc parallel to the Semidiameter drawn to the other Extremity, is in a given *Ratio* to this Semidiameter; and therefore, when an Ellipse is described by a Force directed towards the Centre, that Force is always as the Distance from the Centre. When the Force is directed toward the *Focus*, it is inversely as the Square of the Distance. And these Two Cases are considered particularly, because of their Usefulness in the true Theory of Gravity. To illustrate which, the Laws of centripetal Forces that would cause a Body to descend continually toward the Centre, or ascend from it, are distinguished from those which cause the Body to approach towards the Centre, and recede from it by turns. A Body approaches from the higher Apſid toward the Centre, when its Velocity is less than what is requisite to carry it in a Circle; and if its Velocity increase, while it descends, in a
higher

higher Proportion than the Velocities requisite to carry Bodies in Circles about the same Centre, the Velocity in the lower Part of the Curve may exceed the Velocity in a Circle at the same Distance, and thereby become sufficient to carry off the Body again. But while the Distance decreases, if the Velocities in Circles increase in the same or in a higher Proportion, than the Velocity in a Trajectory can increase, the Body must either continually approach toward the Centre, if it once begin to approach to it, or recede continually from the Centre, if it once begin to ascend from it; and this is the Case, when the centripetal Force increases as the Cube of the Distance decreases, or in a higher Proportion. But though, in such Cases, the Body approach continually towards the Centre, we are not to conclude, that it will always approach to it till it fall into it, or come within any given Distance; for it is demonstrated afterwards in Art. 879 and 880. that it may approach to the Centre for ever, in a Spiral that never descends to a given Circle described in the same Plane, and that it may recede from it for ever in a Spiral that never arises to a given Altitude. An Example of each Case is given when the centripetal Force is inversely as the Fifth Power of the Distance.

When the Trajectory is described in a *Medium*, let z be to a given Magnitude as the centripetal Force is to the Force by which the same Trajectory could be described in a Void; and if the Area be supposed to flow uniformly, the Resistance will be in the compound *Ratio* of the Fluxion of z , and of the Fluxion of the Curve; and the Density of the Medium (supposing the Resistance to be in the compound *Ratio*

of the Density and of the Square of the Velocity) shall be as the Fluxion of the Logarithm of z directly, and the Fluxion of the Curve inversely. Hence, when any Figure that can be described in a Void by a Force that varies according to any Power of the Distance from the Centre, is described in a Medium, the Density of the Medium must be inversely as the Tangent of the Figure bounded by a Perpendicular at the Centre to the Ray drawn from it to the Point of Contact.

After giving some Properties of the Trajectories that are described by a Body when it gravitates in right Lines perpendicular to a given Surface, and their Application to optical Uses, the Author proceeds to consider the Motion of a Body that gravitates towards several Centres. In such Cases, that Surface is said to be horizontal, which is always perpendicular to the Direction of the Gravity that results from the Composition of the several Forces; and it is shewn, that the Velocity which is acquired by descending from one horizontal Surface to another, is always the same (whether the Body move in right Lines, or in any Curves); the Square of which is measured by the Aggregate of several Areas which have the Distances from the respective Centres for their Bases, and right Lines proportional to the Forces at these Distances for their Ordinates.

The Force which acts upon the Moon is resolved into a Force perpendicular to the Plane of the Ecliptic, and a Force parallel to it. This last is again resolved into that which is parallel to the Line of the *Syzigies*, and that which is parallel to the Line joining the *Quadratures*. The First measures the Second

cond Fluxion of the Distance of the Moon from that Plane, the Second and Third measure the Second Fluxions of her Distances from the Line of the Quadratures, and from the Line of the *Syzigies*, respectively. Hence a Construction is derived of the Trajectory which would be described by the Moon about the Earth, in consequence of their unequal Gravitation towards the Sun, if the Gravity of the Moon towards the Earth was as her Distance from it. From this a Computation is deduced of the Motion of the Nodes of the Moon, and of the Variation of the Inclination of the Plane of her Orbit, which we cannot describe here. It is sufficient to observe, that these Motions are found to agree nearly with those which have been deduced from other Theories, and from Astronomical Observations.

A Fluid being supposed to gravitate towards two given Centres with equal and invariable Forces, it is shewn, that the Figure of the Fluid must be that of an oblong Spheroid, and that those two Centres must be the *Foci* of the generating Ellipse. The Nature of the Figure is also shewn, when the Fluid gravitates towards several Centres, or when it revolves on its Axis; but these are mentioned briefly, because such Theories are of little or no Use for discovering the Figures of the Planets.

In the 12th Chapter, the Author proceeds to consider the more concise Methods, by which the Fluxions of Quantities are usually determined, and to deduce general Theorems more immediately applicable to the Resolution of Geometrical and Philosophical Problems. In the Method of Infinitesimals, the Element, by which any Quantity increases or decreases,

is supposed to become infinitely small, and is generally expressed by Two or more Terms, some of which become infinitely less than the rest, and therefore being neglected as of no Importance, the remaining Terms form, what is called the *Difference* of the Quantity proposed. The Terms that are neglected in this manner are the very same which arise in consequence of the Acceleration or Retardation of the generating Motion, during the infinitely small Time in which the Element is generated ; and therefore these Differences are in the same *Ratio* to each other as the generating Motions or Fluxions. Hence the Conclusions in this Method are accurately true, without even an infinitely small Error, and agree with those that are deduced by the Method of Fluxions.

It is usual in this Method to consider a Curve as a Polygon of an infinite Number of Sides, which, being produced, give the Tangents of the Curve, and, by their Inclination to each other, measure its Curvature. But it is necessary in some Cases, if we would avoid Error, to resolve the Element of the Curve into several infinitely small Parts, or even sometimes into Infinitesimals of the Second Order ; and Errors that might otherwise arise in its Application, may, with due Care, be corrected by a proper Use of this Method itself, of which some Instances are given. If we were to suppose, for Example, the least Arc that can be described by a Pendulum to coincide with its Chord, the Time of the Vibration derived from this Supposition will be found erroneous ; but by resolving that Arc into more and more infinitely small Parts, we approach to the true
Time

Time in which it is described. By supposing the Tangent of the Curve to be the Production of the rectilineal Element of the Curve, the Subtense of the Angle of Contact is found equal to the Second Difference or Fluxion of the Ordinate; but in this Inquiry, the Tangent ought to be supposed to be equally inclined to the two Elements of the Curve that terminate at the Point of Contact; and then the Subtense of the Angle of Contact will be found equal to half the Second Difference of the Ordinate, which is its true Value.

Sir *Isaac Newton*, however, investigates the Fluxions of Quantities in a more unexceptionable manner. He first determines the finite simultaneous Increments of the Eluents, and, by comparing them, investigates the *Ratio* that is the Limit of the various Proportions which they bear to each other, while he supposes them to decrease together till they vanish. When the generating Motions are variable, the *Ratio* of the simultaneous Increments that are generated from any Term, is expressed by several Quantities, some of which arise from the *Ratio* of the generating Motions at that Term, and others from the subsequent Acceleration or Retardation of these Motions. While the Increments are supposed to be diminished, the former remain invariable, but the latter decrease continually, and vanish with the Increments; and hence the Limit of the variable *Ratio* of the Increments (or their ultimate *Ratio*) gives the precise *Ratio* of the generating Motions or Fluxions. Most of the Propositions in the preceding Chapters may be more briefly demonstrated by this Method, (of which
several,

Several Examples are given) and the Author makes always use of it in the Sequel of this Book.

It is one of the great Advantages of this Method, that it suggests general Theorems for the Resolution of Problems, which may be readily applied as there is occasion for them. Our Author proceeds to treat of these, and first of such as relate to the Centre of Gravity and its Motion. In any System of Bodies, the Sum of their Motions, estimated in a given Direction, is the same as if all the Bodies were united in their common Centre of Gravity. If the Motion of all the Bodies is uniform and rectilinear, the Centre of Gravity is either quiescent, or its Motion is uniform and rectilinear. When Action is equal to Reaction, the State of the Centre of Gravity is never affected by the Collisions of the Bodies, or by their attracting or repelling each other mutually. It is not, however, the Sum of the absolute Motions of the Bodies that is preserved invariable in consequence of the Equality of the Action and Reaction, as they seem to imagine, who tell us, that this Sum is unalterable by the Collisions of Bodies, and that this follows so evidently from the Equality of Action and Reaction, that to endeavour to demonstrate it would serve only to render it more obscure. On this Occasion the Author illustrates an Argument which he had proposed in a Piece that obtained the Prize proposed by the *Royal Academy of Sciences at Paris* in 1724. against the Mensuration of the Forces of Bodies by the Square of the Velocities, shewing that if this Doctrine was admitted, the same Power or Agent, exerting the same Effort, would produce more Force in the same Body when in a Space carried uniformly forwards,

forwards, than if the Space was at Rest; or that Springs acting equally on Two equal Bodies in such a Space, would produce unequal Changes in the Forces of those Bodies.

Various Problems concerning the Collision of Bodies are resolved in a more general manner than usual. Mr. *Bernouilli* had determined the Motions when the Elasticity is perfect, and One Body strikes Two equal Bodies in Directions that form equal Angles with its Direction; or when there are any Number of Bodies impelled by it on one Side in various Directions, providing equal Bodies be impelled by it on the other Side, in Directions equally inclined to its own Direction. But the Problem is resolved here without these Limitations; some others of this kind are subjoined, and this Doctrine is applied for determining the Motions of Bodies that act upon each other while they descend by their Gravity.

The general Principle derived from these Inquiries is, that if there be no Collision, or sudden Communication of Motion from one Body to another, while they descend together, and in any case, if the Elasticity be perfect, the Sum of the Products, when each Body is multiplied by the Square of the Velocity acquired by it, is the same as if all the Bodies had descended freely from the same respective Altitudes to their several Places; only in collecting that Sum, if any Body is made to ascend, the Product of it multiplied by the Square of its Velocity is to be subtracted: And if the Bodies be supposed to ascend from their Places with the respective Velocities acquired by them, then their common Centre of Gravity will rise to the same.

same Level from which it descended. In other Cases, however, the Ascent of the Centre of Gravity will be less than its Descent, but is never greater.

After demonstrating the usual Rule for finding the Centre of Oscillation, the Author treats of the Motion of Water issuing from a cylindric Vessel. The Effect of the Gravitation of the whole Mass of Water is considered as Threefold. It accelerates, for some time at least, the Motion with which the Water in the Vessel descends; it generates the Excess of the Motion with which the Water issues at the Orifice above the Motion which it had in common with the rest of the Water; and it acts on the Bottom of the Vessel at the same time. Then supposing the last Two Parts of the Force to be in any invariable *Ratio* to each other, when the Diameters of the Base and Orifice are given, he determines by Logarithms the Velocity with which the Water issues at the Orifice; and shews that this Velocity will approach very near to its utmost Limit in an exceeding small Time. When the Water is supposed to be supplied in a Cylinder, so as to stand always at the same Altitude above the Orifice, there is an Analogy between the Acceleration of the Motion of the Water that issues at the Orifice, and the Acceleration of a Body that descends by its Gravity in a Medium which resists in the duplicate *Ratio* of the Velocity. For when the utmost Velocities, or Limits, are equal in those two Cases, the Time in which the issuing Water acquires any lesser Velocity, is to the Time in which the descending Body acquires the same Velocity as the Area of the Orifice to the Area of the Base; and if a cylindric Column be supposed to
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be erected on the Orifice equal to the Quantity of Water that issues at the Orifice in the former of those Times, the Height of this Column will be to the Space described by the descending Body in the latter Time, in the same *Ratio* as the Orifice to the Area of the Base. The *Ratio* of the Force that acts on the Bottom of the Vessel to the Force that generates the Motion of the Water issuing at the Orifice, is deduced from Sir *Isaac Newton's* Cataract, and is the same that follows from the Principle concerning the Equality of the Ascent and Descent of the Centre of Gravity, which was first applied to this Inquiry by Mr. *Daniel Bernouilli* Comment. *Acad. Petrop. Tom.* 2. But there are several Precautions to be taken in applying this Doctrine.

After some other Theorems concerning the Centre of Gravity, and several Observations concerning the Curvature of Lines, and the Angles of Contact; the Author represents four general Propositions in one View, that the Analogy between them may appear. The First gives the Property of the Trajectories that are described by any centripetal Forces, how variable soever these Forces, or their Directions, may be. The Second gives a like general Property of the Lines of swiftest Descent. The Third gives the Property of the Line that is described in less time than any other of an equal Perimeter. And the Fourth gives the Property of the Figure that is assumed by a flexible Line or Chain, in consequence of any such Forces acting upon it. If we suppose a Body to set out from any Point in the Trajectory, or in the Line of swiftest Descent, with the Velocity which it has acquired there, and to move in the right Line which is the

Direction of the Gravity, that results from the Composition of the centripetal Forces, then shall its Velocity, and its Distance from the Point where the Perpendicular from the Centre of Curvature meets that right Line, flow *proportionally*, *i. e.* the Fluxion of the Velocity (or of the right Line that measures it) shall be to the Velocity as the Fluxion of that Distance is to the Distance. When the Velocity and Direction of the Motion is the same in the Line of swiftest Descent as in the Trajectory, their Curvature is the same. Thus in the common Hypothesis of Gravity, the Curvature in the Cycloid, the Line of swiftest Descent, is the same as the *Parabola* described by a Projectile, if the Velocities in those Lines be equal, and their Tangents be equally inclined to the Horizon. In order to find the Nature of the *Catenaria* in any Hypothesis of Gravity, suppose the Gravity to be increased or diminished in the same Proportion as the Thickness of the Chain varies, and to have its Direction changed into the opposite Direction; then imagine a Body to set out with a just Velocity from a given Point in the Chain, and to describe the Curve. The Tension of the Chain at any Point will be always as the Square of the Velocity acquired at that Point, and if a Body be projected with this Velocity in the Direction of the Tangent, the Curvature of the Trajectory described by it will be one Half of the Curvature of the Chain at that Point. We must refer to the Book for a fuller Account of these and of other Theorems.

In the XIIIth Chapter, the Problems concerning the Lines of swiftest Descent, the Figures which amongst all those that have equal Perimeters produce

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Maxima or *Minima*, and the Solid of least Resistance, are resolved without Computations, from the first Fluxions only. There are also easy synthetic Demonstrations subjoined, because this Theory is commonly esteemed of an abstruse Nature, and Mistakes have been more frequently committed in the Prosecution of it, than of any other relating to Fluxions. To give some Idea of the Author's Method, suppose the Gravity to act in parallel Lines, a to denote the Velocity acquired at the lowermost Point of the Curve, and u the Velocity acquired at any other Point of the Curve. Suppose the Element of the Curve to be described by this Velocity u , but the Element of the Base to be always described by the constant Velocity a . Then it is easily demonstrated without any Computation, that the Element of the Ordinate being given, the Difference of the Times in which the Elements of the Curve and Base are thus described is a *Minimum*, when the *Ratio* of those Elements is that of a to u ; *i. e.* when the Sine of the Angle, in which the Ordinate intersects the Curve, is to the *Radius* in this *Ratio*. Supposing therefore this Property to take place over all the Curve, the Excess of the Time in which it is described by the Body descending alongst it, above the Time in which the Base is described uniformly with the Velocity a , must be a *Minimum*; and this latter Time being given, it follows that the Time of Descent in this Curve is a *Minimum*. When the Gravity tends to a given Centre, substitute an Arc of a Circle described from that Centre through the lowermost Point of the Curve in the Place of the Base in the former Case; and the Property of the Line of swiftest Descent will be discovered in the

same manner. The Nature of the Line that among all those of the same Perimeter is described in the least Time, is discovered with great Facility, by determining from the former Case the Property of the Figure when the Sum or Difference of the Time in which it is described by the descending Body, and of the Time in which it would be described by any given uniform Motion, is a *Minimum*; for the latter Time being the same in all Curves of the same Length, it follows that the Figure, which has this Property, must be described in less Time than any other of an equal Perimeter. The general Isoperimetrical Problems are resolved, and the Solutions are rendered more general, with like Facility by the same Method; which is also applied for determining the Property of the Solid of least Resistance, and serves for resolving the Problem, when Limitations are added concerning the Capacity of the Solid, or the Surface that bounds it.

The last Chapter of the First Book treats chiefly of Gravitation towards Spheroids, of the Figure of the Planets, and of the Tides. The Author, having Occasion in those Inquiries for several new Properties of the Ellipse, begins this Chapter by deriving its Properties from those of the Circle, by considering it as the oblique Section of a Cylinder, or as the Projection of the Circle by parallel Rays upon a Plane oblique to the Circle. In this manner the Properties are briefly transferred from the one to the other, because by this Projection the Centre of the Circle gives the Centre of the Ellipse; Diameters perpendicular to each other in the Circle with their Ordinates, and the circumscribed Square, give conjugate Diameters of the
the

the Ellipse with their Ordinates, and the circumscribed Parallelogram; parallel Lines in the Plane of this Circle are projected by Parallels in the Plane of the Ellipse that are in the same *Ratio*; any Area in the former is projected by an Area in the latter, which is in an invariable *Ratio* to it; and concentric Circles give similar concentric Ellipses. It is likewise shewn how Properties of a certain kind are briefly transferred from the Circle to any conic Section with the same Facility.

After demonstrating the Properties of the Ellipse, it is shewn, that if the Gravity of any Particle of a Spheroid being resolved into two Forces, one perpendicular to the Axis of the Solid, the other perpendicular to the Plain of its Equator, then all Particles, equally distant from the Axis, must tend towards it with equal Forces; and all Particles at equal Distances from the Plain of the Equator, gravitate equally towards this Plain; but that the Forces with which Particles at different Distances from the Axis tend towards it, are as the Distances; and that the same is to be said of the Forces with which they tend towards the Plain of the Equator.

From this it is demonstrated, that when the Particles of a fluid Spheroid of an uniform Density gravitate towards each other with Forces that are inversely as the Squares of their Distances, and at the same time any other Powers act on the Particles, either in right Lines perpendicular to the Axis, that vary in the same Proportion as the Distances from the Axis, or in right Lines perpendicular to the Plain of the Equator, that vary as their Distances from it, or when any Powers act on the Particles of the Spheroid, that may be resolved into Forces of this kind; then the
Fluid :

Fluid will be every-where *in Equilibrio*, if the whole Force that acts at the Pole be to the whole Force that acts at the Circumference of the Equator, as the Semidiameter of the Equator to the Semiaxis of the Spheroid; and that the Forces with which equal Particles at the Surface tend towards the Spheroid, will be in the same Proportion as Perpendiculars to its Surface, terminated either by the Plane of the Equator, or by the Axis. Because the centrifugal Force with which any Particle of the Spheroid endeavours to recede from its Axis, in consequence of the diurnal Rotation, is as the Distance from the Axis, it appears that if the Earth, or any other Planet, was fluid, and of an uniform Density, the Figure which it would assume would be accurately that of an oblate Spheroid generated by an Ellipsis revolving about its Second Axis.

Afterwards the Gravity towards an oblate Spheroid is accurately measured by circular Arcs, not only at the Pole, but also at the Equator, and in any intermediate Places; and the Gravity towards an oblong Spheroid is measured by Logarithms. The Gravity at any Distance in the Axis of the Spheroid, or in the Plane of the Equator produced, is likewise accurately determined by like Measures, without any new Computation or Quadrature, by shewing that when Two Spheroids have the same Centre and *Focus*, and are of an uniform Density, the Gravities towards them at the same Point in the Axis or Plane of the Equator produced, are as the Quantities of Matter in the Solids.

This Theory is applied for determining the Figure of the Earth, by comparing the Force of Gravity in
any

any given Latitude, derived from the Length of a *Pendulum* that vibrates there in a Second of Time, with the centrifugal Force at the Equator, deduced from the periodic Time of the diurnal Rotation, and the Amplitude of a Degree of the Meridian; or by comparing the Lengths of *Pendulums* that vibrate in equal Times in given unequal Latitudes; or by comparing different Degrees measured upon the Meridian. By the best Observations it would seem, that there is a greater Increase of Gravitation, and of the Degrees of the Meridian from the Equator towards the Poles, than ought to arise from the Supposition of an uniform Density. Therefore the Author supposes the Density to vary from the Surface towards the Centre; and, in several Cases he has considered, he finds that a greater Density towards the Centre would account for a greater Increase of Gravitation towards the Poles, but not for a greater Increase of the Degrees of the Meridian; and that the Hypothesis of a less Density towards the Centre would account for the latter, but not for the former, supposing (after Sir *Isaac Newton*) the Columns of the Fluid to extend from the Surface to the Centre, and there to sustain each other. On this Account he determines the Gravitation towards the Earth, when it is supposed to be hollow with a *Nucleus* included, according to the Hypothesis advanced by Dr. *Halley*, with the Difference of the Semidiameters that might arise from such a Disposition of the internal Parts. But in this Case, and when the Density is supposed variable, the spheroidical Figure is only assumed as an Hypothesis. He adds, that by imagining the Density to be greater in the Axis than in the Plain of the Equator at equal

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Distances from the Centre, an Hypothesis perhaps might be found, that would account for most of the *Phænomena*; but that a Series of many exact Observations is requisite, before we can examine with any Certainty the various Suppositions that may be imagined concerning the internal Constitution of the Earth. This Doctrine is likewise applied for determining the Figure of *Jupiter*.

It follows from the same Theorem, that if we suppose the Earth to be fluid, and abstract from its Motion upon its Axis, and the Inclination of the right Lines in which its Particles gravitate towards the Sun or Moon, the Figure which it would assume in consequence of the unequal Gravitation of its Particles towards either of those Bodies would be accurately that of an oblong Spheroid having its Axis directed towards that Body. The Ascent of the Water, deduced from this Theorem, agrees nearly with that which Sir *Isaac Newton* found, by computing it briefly from what he had demonstrated concerning the Figure of the Earth. Several Observations are subjoined concerning the Tides, and the Causes which may contribute to increase or diminish them, particularly the Inequality of the Velocities with which Bodies revolve about the Axis of the Earth in different Latitudes.

This Chapter concludes by demonstrating briefly, that if the Attraction of the Particles decreased as the Cube of their Distance increases, or in any higher Proportion, then any Particle would tend towards the least Portion of Matter in Contact with it, with a greater Force than towards the greatest Body at any Distance, how small soever from it. The true
Law

Law of Gravity is better adapted for holding the Parts of each Body in a proper Union, while it perpetuates the Motions in the great System about the Sun, and preserves the Revolutions in the lesser Systems nearly regular; and the Author concludes with observing, that a remarkable geometrical Simplicity is often found in the Conclusions that are derived from it.

An Account of Book II. will be given in the next Transaction.

VII. *De Calculo prægrandi a Muliere cum Urina excreto Observatio Dⁿⁱ Antonii Leprotti, R. S. S. Pont. Max. Archiat. per Abbatem Didacum de Revillas, R. S. S. ad D. Smart Lethieullier R. S. S. transmissa.*

Romæ, pridie Cal. Januarii An. 1743.
 Read Jan. 27. 1742-3. **Q**UÆ quindecim abhinc annis Vidua est Mulier pauper quinquagesimum agens annum summa urinæ difficultate per quadraginta Menses laborabat; quum nocte ei supervenit mictus, imo sinceri cruoris profluvium ad tres circiter libras; simulque Lapis, ejus, quæ in adjecta Figura describitur, formæ & molis, extrusus est, cujus pondus jam exsiccati uncias duas & grana novem ac viginti exæquat*. Mulier autem ingenti per eos menses gravitatis sensu, assiduoque dolore, ad Vesicæ cervicem afficiebatur sive cubans sive crecta; nunc autem solido exacto mense, ea invita, prodere pergunt cum sanie urinæ. *Vide Fig. II. in TAB. I.*

unc. den. gr.
 * i. e. 1 : 17 : 4. TR.

VIII. *Description of a Machine for dressing and curing Patients, who are very unwieldy, and are under the Surgeon's Hands for some Ailment on the Back, the Os Sacrum, &c. or are apprehensive of it. By M. le Cat, F. R. S. Surgeon to the Hotel Dieu at Rouan, and Royal Demonstrator in Anatomy and Surgery: Abstracted from the French by P. H. Z. F. R. S.*

Read Feb. 3.
1742-3.

A Lusty Body labours, as it were; under the Richness of its Constitution, which at the long run turns to Misery: The Vessels of a plethoric Body are, even in the most vigorous State of it, hardly able to convey all the Juices; but when that Vigour is lost, they stagnate and corrupt, and produce numberless Distempers: If any critical and salutary Evacuations free it of Part of its Burden, there remain flabby Bags and Cells ouzing Humours, which become Materials for Imposthumes, for want of a proper Supply of Animal Spirits, and laudable Humours, which are compressed and stopped by the Weight of the respective Parts. The increasing Weakness of the Patient hinders him from stirring, and putting himself into the Situation necessary for his Cure: His enormous Bulk makes it even impossible for his Attendants to assist him; the Number of Hands that are then employed, rather give him Torment than Ease, and the Apprehension of changing his Posture at so painful a Rate, will make him rather

rather prefer an easy Situation, that will at last lead him to the Grave.

Since my practising Surgery, I have had several of those unhappy Persons under my Hands, and even some who were dearer to me than the rest of my Patients; and I have had the Grief to see them carried off in despite of all the Resources my Attachment furnished me with, and those my Profession suggested to me then, as generally used. Finding these latter insufficient by repeated Experience, my Imagination at last made me conceive a sort of *hanging Cradle* or *Hammock*, as represented in the Figure hereto annexed. In *January 1741*. I gave the Draught of this Machine to some Workmen, having then under my Hands the Abbé *de la Buaille* of this City of *Roüan*, a Person of a vast Bulk, paralytic, and labouring under a Mortification about the *Os Sacrum*. The following Explanation of the Figures will shew the several Uses of the Machine.

TAB. 2. represents the Patient's Bed-chamber with a Bed in it without the Bedding, in order the better to shew the Machine.

Upon it lies a sort of Boat of *Turkey Leather*, full as long as the Bed, with very strong Hems all round, and Eilet-holes for receiving Hooks, that serve to lift up this *Hammock*.

The Hooks are fastened to several Ropes, all which depend on as many Cross-beams of very solid Wood.

The Cross-beams consist of one Beam of the Length of the whole Bed, running Lengthways over the Middle of it, and Four transverse Beams, the Two middlemost of which are somewhat longer than the

others. The Ropes on which the *Hammock* hangs, are fastened to the Extremities of these Beams, which keep the *Hammock* displayed; and on the same Extremities are also fastened all the Ropes, which unite in one that passes through the Testern of the Bed, and above it hangs on a Pulley, that is fixed to the Cieling of the Bed-chamber.

Another Rope that is run into the Pulley, passes into another Pulley corresponding to it, hanging at some Distance from the Bed, where a Man is placed to pull it, and raise the *Hammock*.

What we chiefly intend in Dressing a Patient in Question, are,

1st, To dress and refresh him, that is to say, gently to place him in a proper Posture, easy both for himself, and those who attend him.

2^{dly}, To put him into an easy Situation, that may also promote his Recovery: The making of his Bed often, is already of great Ease to him; but at the same time it is necessary, that his Wounds or Ailments may not bear upon any the least thing possible; and therefore his Bed ought to be composed of several small Matresses, or of Matresses of several Pieces, each with its Tick over it; these Matresses ought besides to be supplied with Numbers of Pillows, each with its Pillow-bier, so that he who waits on the Patient, may place them where it is proper, for the Ease of the Person, and of the Part affected. Nothing is more proper for this Purpose than our *Hammock*; the Patient may be lifted up from his Bed, and suspended just above those Pillows, and higher yet, if necessary.

Our *Hammock*, being of *Turkey Leather*, fits itself to those Pillows, and gathers them in as the lower Sheets would do; but the Inconveniency of Sheets we have supplied with those Ticks and Pillow-biers covering the Matresses and Pillows.

The *Turkey Leather* of the *Hammocks* is full wide, not only to cover the whole Bed, but even so as that the Hems or Borders of it may hang down round about it, and tuck in under the Matresses: The Bottom of it is pierced in those Places which answer to the *Anus*, or any Part affected, so that the Evacuations may find their Passage into Receptracles between the Pillows ranged accordingly.

When the Patient is to be dressed or refreshed, the Borders of the *Hammock* are taken up, and the several Hooks passed through, by which he is to be suspended, as appears in the Figure; and then a Man, being placed at the Rope that runs over the Pulleys, lifts the Patient up to the Height necessary for the Surgeon to search and dress the Wound, and for the Assistants to make his Bed, which, even for the greater Conveniency, may be pulled out from under the *Hammock*.

When all is done, the Bed is pushed back again to its former Place, the Patient is gently let down upon it, the Cross-beams are lowered and detached both from the *Hammock* and the Block, and put out of the Way into a Corner of the Room; instead of it, a Rope is fixed to the Hook of the Block, tied into an Eilet at the End, coming down towards the Bed within the Patient's Reach, in order to help himself when he wants to stir a little.

The *Hammock* being displayed, and the Cross-beams taken away, the Patient is wrapped up in Napkins as much as possible, to supply the Sheet he wants between his Body and the Leather of the *Hammock*; he is afterwards covered with an upper Sheet, and other necessary Bed-cloaths.

TAB. 3. This Machine may be farther improved by Use. For Instance: Since I contrived this, I thought that instead of the Border or Hem of the *Hammock*, one might make strong cylindrical iron Rods, like Curtain-rods, formed into a Square, somewhat larger than the Bedstead, to the Four Corners of which are fastened as many Ropes, which meet at the Pulley; in which Case the Cross-beams, and the Ropes depending on them, become useless; and instead of a *Hammock* all of one Piece, one might fix Four broad Straps of *Turkey* Leather to Two Sides of the square Rod, which may be placed under such Parts of the Patient's Body as will be proper, and which leave a Space between each other where it is convenient. These Straps may be fastened to the iron Rods by several Buckles with Rings to slide along the Rods, by the Help of which the Straps may be pushed on to such Places where there is Occasion; they may also thereby be stretched or slackened, or even be taken off, or changed as is thought fit. After the Patient has been dressed, and the Bed made, the Four Ropes may be taken off both from the Rod and from the Block, and the Rod be let drop with the Extremities of the Straps down upon the Floor round the Bedstead, which being narrower than the Square of the Rod, the latter will easily slip over it.

I have given these two Methods together, as there may be Occasions when one becomes preferable to the other.

IX. *An Account of a Treatise, (intituled, D. Alberti Halleri Archiatri Regii & Elect. Medicin. Anatomiae, Botan. Praelect. &c. Enumeratio Methodica Stirpium Helvetiae indigenarum. Qua omnium brevis Descriptio & Synonymia, Compendium Virium Medicarum, dubiarum Declaratio, novarum & rariorum uberior Historia & Icones continentur. Gottingiae, 1742. in Folio) extracted and translated from the Latin by William Watson, F. R. S.*

Read Feb. 3.
1742-3.

THIS learned and ingenious Performance, in Two Volumes in Folio, contains a Dedication to the PRINCE of WALES, a Preface, and 794 Pages.

In the Preface, the Author first lays down a Geographical Account of *Switzerland*, being situate from 46 to 48 Degrees in Latitude, and Four Degrees in Longitude; then mentions its various and almost surprising Degrees of Heat and Cold within the Space of a few Miles, arising from the different Arrangement of the Mountains: That it is in some Parts destitute both of Corn and Wood from the Intensity of the Cold; in others, where there are high Moun-

Mountains to the North, and Openings to the South, the reflected Heat becomes so troublesome, that the Inhabitants are forced to desert the Towns, and take Shelter in the Woods; that in other Parts the Country is so extremely pleasant, that *Tavernier* himself, though he had travelled over great Part of the Globe, declared he never had observed any more beautiful. The Author then mentions, that the Plants produced in such Difference of Soils and Situations, must be very numerous; he accordingly met with not only many of the Plants of the warmer Parts of *France*, almost all the *German* ones, but even those of *Lapland* and *Spitzberg*. These Varieties have been collected in a short time. You gather in the same Day, the *Bistorts* and *Saxifrages* which *Martens* collected in *Spitzberg*; the yellow *Milfoil*, *Xeranthemum*, *Ephedra*, and other Ornaments of the Southern Part of *France*. The Progression between both Extremes is so regular, that in going from *Bern* to *Grimsul*, you pass first by *Chestnut-trees*, and other Inhabitants of the warm Countries, then *Vines*, then *Walnut-trees*, then *Beeches* and *Oaks*, then *Firs*, then *Larch-trees*, then *Pines*, then barren Heaths producing *Whortle-berries* and such-like, then Rocks, and Plants a Span high, and last of all, beyond which Vegetation ceases, you meet with a Species of round-leaved *Willow*, not exceeding an Inch in Height, and the hairy *Crow-foot* of *Platerus*; beyond this District, the Tops of the Mountains are covered with Snow. This great Variety is not (as it must be in any other Part of the World) the Collection of many Provinces, but furnished within the Space of 17 Leagues; and would be still less, if in going from

Se-

Sedunum, you ascend Mount *Sanetch*, whose Top is but seven Leagues from *Sedunum* [or *Syon*].

The Author adds, that the Sides of the Mountains produce great Variety of Mosses and Fungus's, that the Pastures furnish an inexpressible Collection of Grasses, of which in this Book he mentions 220 Species. The following Kinds of Plants seem to be wanting in *Switzerland*, viz. the hotter Kind, such as *Thyme*, *Lavender*, and *Rosemary*; those very frequent in champain Countries; those which are produced in Bogs and putrid Soil; some of those peculiar to the North, and maritime Plants.

The *Alps* contain about 500 Species peculiar to themselves, all diverse, perennial most of them, biting, scented, and frequently with a white Flower; besides many Plants common to other Places.

The Author then proceeds to enumerate all the Botanists, who by their Journals and Publications have laboured to oblige the World with Histories and Descriptions of the almost inexhaustible Number of Plants, which the various Soil and Situation of this Country produces; and after having mentioned the Performances of these great Men, he gives some Account of his own Travels, and the Progress of his botanical Studies; that he had gone through *Germany*, *Holland*, *France*, and *England*, and made very few Observations of the botanic kind, at least had preserved no Specimens of what he had seen; but upon his Arrival at *Basil* to attend the Lectures of *Bernoulli*, and study Mathematics there, he was seized, as it were, by the Genius of the Place, where those great Writers the *Bauhins* had lived, and were public Professors; and whose Chair at that Time

was very worthily filled by *Starbelinus*: That he began to collect, describe, and compile, with so great Hopes of Success, however remote, that he even attempted the Work before us, at a time when he was scarcely acquainted with the more common Plants. A Work of this kind had been begun by *John Gesner* of *Zurich*, a Descendant of the famous *Conrad Gesner*'s, and a Friend of our Author's, for which Task he was very well qualified by his many laborious Researches; but at length his bad State of Health would not permit him to proceed in a Science, where he must not be confined only to his Closet, but climb up almost inaccessible Mountains, sometimes nearly perishing with Cold, and, possibly, in the same Day, almost stifled with Heat. This Gentleman not only sent our Author a great many Plants, but granted him whatever he had occasion for of his Collection, which consisted of a great Number of valuable Specimens, of which he alone was possessed.

Our Author specifies likewise, what Parts of the *Alps* have been looked over, and what remain hitherto unattempted; and then shews how large a Field is yet behind, for future Botanists to exercise their Genius upon; and that these Mountains have rather been cursorily passed over by Persons travelling over them to remote Places, often at an improper Season, than carefully examined; from whence it happens, that many not only of Mosses, but of the most perfect Plants, have either been omitted, or so negligently described, that it is impossible to reduce them to the Genus to which they belong. Add to this, that the *Fungus*'s, and the very small Plants, such as the *Centunculus*, *Sedum tetrapetalon*, &c. were overlooked

looked by the antient Botanists, and seem to have been reserved for the Industry and Perspicuity of the *Vaillant's*, *Dillenius's*, and *Micheli's*, of the present Age. Our Author then candidly confesses, that although he had herborized upon many different Parts of this Country for Nine Years, he could by no means promise a full and perfect Enumeration of its Plants; for the Descriptions of the more antient Writers, especially the Grasses mentioned by *Caspar Bauhin*, are so obscure, that it is scarcely possible to know many of them by those means; that some Plants are inserted by Authors, which have occurred to no body since their Time; that others, if not quite lost, he never could meet with, notwithstanding he travelled for that Purpose to the Places where they have been said to be found; which may in some measure be owing to our Author's being short-sighted, from which Defect (he believes) he may, no doubt, have passed over some Plants, which he had been particularly in quest of: That he had received some Specimens so ill preserved, as not to be able to discover their generic Marks; and, lastly, that it is almost impossible to save any Seeds of the Plants on the *Alps*, or see them in that State, on account of the Snows falling so early as the latter End of *August*, and Beginning of *September*, whereby the Mountains are covered, and rendered unpassable.

Surrounded with Difficulties, he despairs of perfecting his Catalogue; but hopes he shall have the Reader's Pardon, after he is apprised of the Means our Author took against them. First, he carefully marked out the Characters of all his Plants, the Day he collected them; for not being prejudiced in favour

of any artificial Method, he looked over the Composition of the Flower with regard to its Petals, *Calyx*, and Seed-vessel, after the manner of Professor *Boerhaave*, at a Time when nobody had considered the *Stamina* and *Tubes* as generical Notes: That he compared with his Plants the botanical Writers of more than Two Centuries, whose Names are mentioned at the End of the Preface, beginning in order of Time with *Brunfelsius*, and concluding with the late Work of *Monsieur Geoffroy*: That he had examined their Descriptions of Plants, and compared them with their Figures, and made himself a Pinax of the Plants of *Switzerland*, even to the present Time.

Our Author, in the Work before us, has never inserted a doubtful Plant, without mentioning his Scruple, nor any but what he himself has seen, without an Asterisk. He has added to the End of the Work, those which he could with but little Certainty refer to any Class; and, contrary to the Practice of some late Writers, he never enumerates Variety, nor ever regards Proportion as a Mark of specific Difference, if even a less Plant produces Flowers twice as big as a larger Plant of the same Species, and holds that Size when planted in Gardens, and continues the Difference to its Posterity.

As to the Method and Order of this Work, our Author has been as short, and at the same time as descriptive, as possible. He has given the Synonymes of most good Authors: He generally first affixes the Name the Discoverer gave it, unless a very improper one; and then proceeding usually as the Authors lived, sets down the Appellations of *Conrad Gesner*,

Gesner, Cordus, Dodoneus, Lobel, Tabernemontanus, Hortus Aichstadiensis of *Basil Besler, Clusius, Caspar* and *John Bauhin, Morriſſon, Tournefort*, and others, who have lived ſince them, either as they have diſcovered a new Plant, or illuſtrated an old one with a new Deſcription, Figure, or Character. With regard to Method, he ſays he might have diſpoſed them alphabetically, followed *Boerhaave's* Method, or *Linneus's*, but was fearful of making unnatural Diſtortions; eſpecially as he was not writing an univerſal Hiſtory of Plants. He thinks it not at all proper to diſpoſe Plants in the ſame Claſs, unleſs their Affinity is perfect; and lays it down as a Foundation, upon which alone a natural Method can be formed, that however different Plants may ſeem in one Characteriſtic, thoſe ſhould be placed together which agree in moſt others; and however alike they may be in one Point, that thoſe be ſeparated which differ in many others. The Neglect of this Axiom has made all Methods unnatural.

The Author then apologizes for giving new Names to ſome Plants, but he could not omit inſerting ſome that were more expreſſive, and give them the Preference to old ones, that imported little or nothing, although they had the Sanction of Antiquity; but he ſcarce ever has changed the generical Names, becauſe amongſt things that are in themſelves indifferent, Cuſtom ſhould be always complied with; and as all generical Names are arbitrary, ſcarce any can be thought of to contain enough to diſtinguiſh the Plant by; but the ſpecific Name ought to be a ſhort Definition and Compendium of its niceſt Differences: And although this may ſometimes be thought too long,

long, the Marks of Difference in many kinds will not permit them to be contracted.

After he has acquainted the Reader with his Objections, and told his Reasons, he proceeds to that Part of his Work, which is intituled, *Nomina Scriptorum & Editionum*; and has given a Specimen of his great Erudition in a very laborious and learned History of almost all Botanical Authors, for more than Two Centuries: He therein points out their Excellencies and Defects, shews which of them were Originals, and which Plagiaries; gives an Account of all their Publications and Editions, and deduces the Rise and Progress of Botany through all its Stages, from the general Darknes of the 15th Century, to the nice Distinctions of the present Time. This may be esteemed a very valuable Performance. It may not be improper to exhibit from it, the different Characters of Two Books in the Author's own Words, whereby some Judgment may be formed of the rest. First, mentioning *Clusii rariorum Stirpium per Hispaniam observatarum*, he says, "*Cordus* was restored to the World in *Clusius*. He, with incredible Labour, collected the Plants of *Spain*, *Languedoc*, *England*, the *Alps*, *Austria*, some Parts of *Hungary*, and those about *Frankfort*: He afterwards drew them, and published their Figures very expressively, and with great Neatness. He alone doubled the Number of Plants before known, although indeed many have been attributed to him, which are concealed in the Works of *Cordus*, *Aretius*, and *Gesner*." Our Author afterwards, speaking of *Fabregeon's Description des Plantes qui naissent autour de Paris*, says, "Nothing can be
" more

“ more audacious than this Writer ; he often quar-
 “ rels with good Authors, and obtrudes upon them
 “ long since dead, arbitrary Definitions proposed by
 “ himself. The Synonymes of his Plants, and the
 “ Definitions of his Species, are very much confused :
 “ He takes, with the utmost Impudence, the Names
 “ of *Vaillant* and *Tournefort*, but with ridiculous
 “ Alterations. Besides, to my great Abhorrence, he
 “ inserts a very great Number of Plants most cer-
 “ tainly Exotic, as growing about *Paris* ; and, lest
 “ any thing should be wanting to spoil this Work,
 “ the typographical Errors are infinite.”

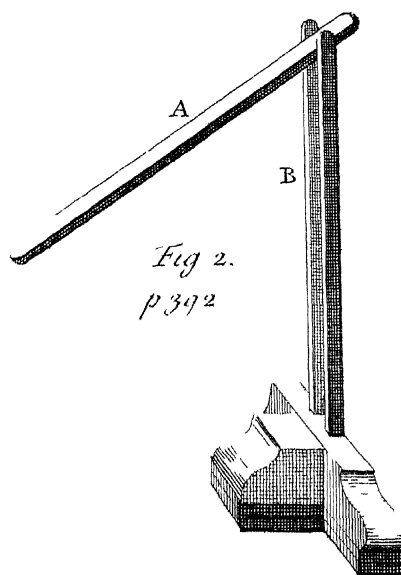
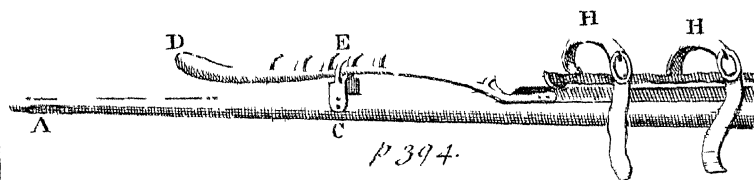
Before I give an Account of our Author's System of Botany, it will be necessary to mention the different Parts of Plants, from which other Authors have formed theirs. *Conrad Gesner* was the first who discovered, that Plants might be distinguished into *Genera* from their different manner of bearing Fruit, as appears by his posthumous Letters published by *Camerarius* ; but *Cesalpinus* first reduced it into Practice. *Cesalpinus*, I say, *Ray*, *Herman*, whose Plan is much improved by *Boerhaave*, and *Knaut's* Systems, are formed from the Fruit ; *Tournefort's*, from the Figure of the Flower ; *Rivinus's*, which is followed by *Ruppius*, from the Number and Equality of the Petals ; *Magnol's*, from the *Calyx* ; *Linneus's*, from the *Stamina*, *Pistillum*, and Sex of the Flower ; and our Author, his principally from the Number and Disposition of the *Stamina*, and likewise from the manner of Fruiting. I have, at the End of this Extract, abridged it according to the Order of the *Classes*, *Genera*, and *Species*.

Throughout the Body of this Work, our Author has ranged his Plants after this Method; and when he mentions a particular Plant, he first gives the generical Name, and its Inventor; then lays down the Form of the Flower, and the Manner of distinguishing this Plant from others of the same Species; then quotes the Synonymes, then the Place of its natural Growth, afterwards the Description of its Root and Leaves; and lastly, collects all the Evidence on both Sides, with regard to its Uses as a Medicine, or the contrary. I think it not improper to give here Part of the History of one Plant, as a Specimen of the rest. The Author, speaking, p. 298. of *Veratrum*, or *white Hellebore* of the Shops, after mentioning the generical Name, Form of the Flower, 29 Synonymes of different Writers, the Place of Growth, and the Form of its Root, says, " This Plant is
 " universally agreed to be hurtful, though *Brassa-*
 " *vola*, p. 531. found some People hardy enough
 " to give a Drachm at a Dose, without any Cor-
 " rector; which Dose even *Welsch* and *Herman*
 " have allowed in Infusion to Dæmoniacks, also
 " *Matthiolus*, p. 1222. with good Success, to Lu-
 " natics. *Hermann* in like Cases gives the Root
 " in Substance, from 15 to 30 Grains. Notwith-
 " standing which, we find in the *Ephemerides na-*
 " *turæ curios.* Anno 1^{mo} Obs. 65. that One Scruple
 " has certainly produced Convulsions; and *Wepfer*
 " mentions a Dog killed with the same Dose, and
 " *Fallopious de Purgant.* likewise many strangled
 " therewith. *Lentilius*, p. 868. takes Notice of
 " violent Vomitings occasioned by the Root's being
 " given by Mistake, instead of *Solomon's-seal*. The
 " *Acta*

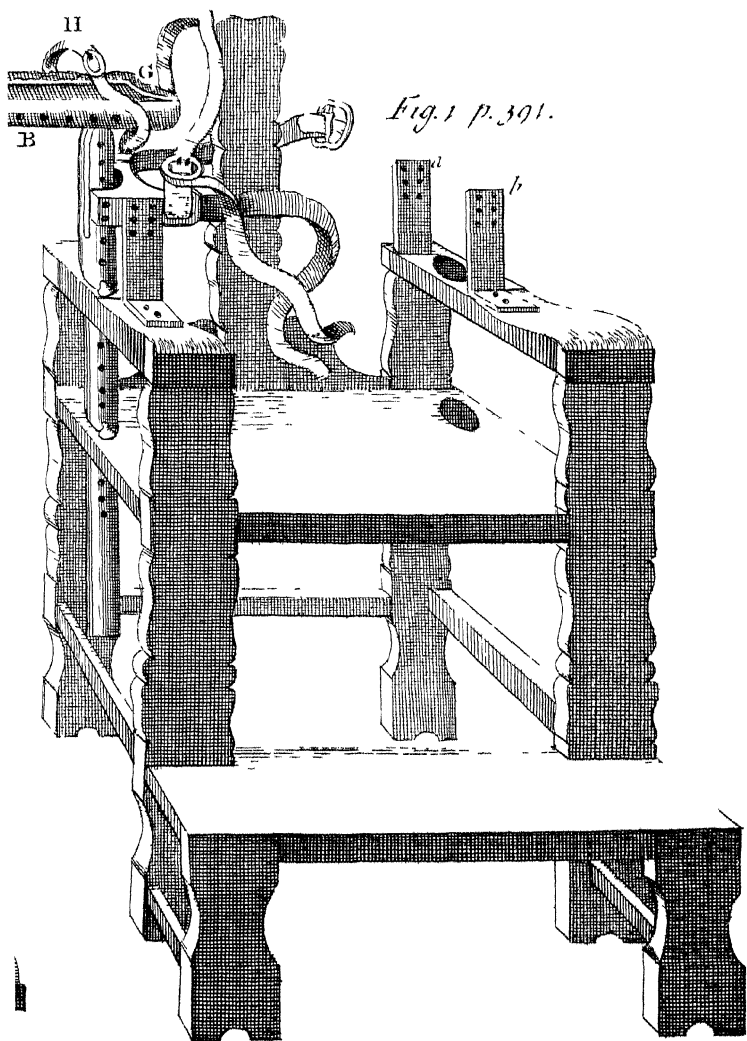
“ *Acta Hafn.* Anno v. Observ. 55. say, that those
 “ most hardy Mortals, who live in the Northern
 “ Parts of the World, and purge themselves with
 “ this Plant, receive great Mischief in their Eyes
 “ therefrom, even sometimes to be followed by
 “ Blindness. See also its terrible Effects in the *Breslau*
 “ *Transactions*, Anno 1725. Even amongst the An-
 “ tients, notwithstanding that both sorts of *Hellebore*
 “ were produced in *Anticyra*, the black was only
 “ made use of in Purges, on account of the emetic
 “ Quality of the white. See *Pausanias*, Lib. X. pag.
 “ 623. *Alleyne* will scarcely admit it to be safe
 “ given in Powder as a Sternutatory. But if the
 “ Juice of this Plant, with its full Powers, shall by
 “ any Method get into the Blood, it is so quick a
 “ Poison, that the Animal immediately dies, if
 “ wounded even in a slight manner, and the Juice
 “ applied thereto. See *Crato*, Epist. II. pag. 226.
 “ *Matthiolus* found this Experiment true upon Hens.
 “ See pag. 1226. *Epistolar.* pag. 219. And that the
 “ Putrefaction excited thereby was so great, that the
 “ Flesh immediately was grown soft. See *Arceus de*
 “ *Curand. Vuln.* Lib. I. pag. 70. Nor does the Cause
 “ seem to be obscure, seeing that the Roots, being
 “ chewed, fire the Mouth and Throat, and pour
 “ forth a very sharp Liqueur, not unlike that of Lime.
 “ *Geoffroy de Mat. Medic.* Vol. II. p. 226. But *Con-*
 “ *rad Gesner*, in the Work published by himself,
 “ assures us, that his *Oxymel Elleboratum* may be
 “ given to Two Drachms without any Mischief, and
 “ that it is very useful to promote the *Menses*, Ex-
 “ pectoration, and Sweat.”

This may serve as a Specimen of our Author's great Industry and Exactness; which he adheres to throughout the whole Work, where a Plant has by any, whether antient or modern, medical Writer or Historian, been celebrated for medicinal Purposes, or its Uses in the Art of Dying. His Descriptions are so exact, that it is almost impossible, that any Person, ever so little conversant with Botany, should mistake one Plant for another. His Figures, of which there are 24 Tables, are finely engraved, and with great Accuracy, as appears from comparing them with their Descriptions. His Method is very natural, and not difficult to comprehend when considered; though at first View it seems more so than *Ray's*, *Tournefort's*, or *Boerhaave's*: And indeed there have been already so many Botanical Systems, such warm Controversies among Authors, so many bad Names, such great Confusion, that as often as there appears a new System, it sends forth a Panic throughout the Botanical World; as it adds to the Number of Names already too great, and tends to the Discouragement of those who are desirous of being acquainted with Plants. But our Author's System being, as I said before, very natural, and as he gives but few new generical Names, and at the same time when he gives his own, mentions those of most good Authors; these Considerations take off many Objections, to which some late Botanic Writers are liable. In the Work before us, the Author takes in only the Plants of *Switzerland*; but I believe his Plan may be extended to a general History, which, if executed with the same Accuracy as the present Work, cannot but be a most valuable Performance.

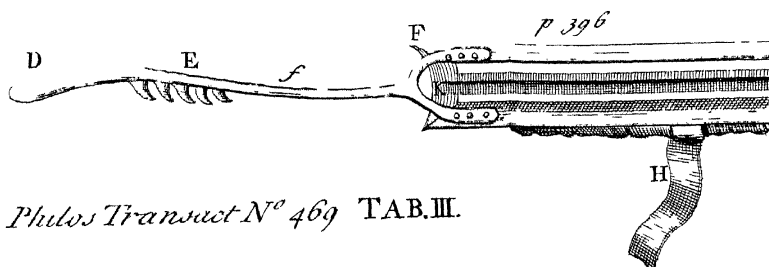
I. Sun vel, II.	flore, &c - - - - -	Staminibus &c petalis destitutæ, femine solo donatæ; ut <i>Coniferae</i> . Staminibus veris &c petalis destitutæ, flore aliquo &c femine donatæ	1. Stamini- bus omni- bus nullis, ut <i>Lichen</i> . 2. Epiphyllis, ut <i>Osmunda</i> , <i>Polypodium</i> . 3. Stamini- bus coalis- a fructu remotis, confisere, ut <i>Salix</i> . 4. Non Julifere, ifostemones, ut <i>Alchimilla</i> . 5. Meioftemones; ut <i>Kyrtel</i> . 6. Diploftemones; ut <i>Tribus</i> . 7. Aquaticæ variae; ut <i>Chara</i> , <i>Limn</i> . 8. Triftemones, flore plerumque bifol 9. Graminibus adnatis; ut <i>Cyperoides</i> .
III.	Petalis destitutæ, femi- veris flaminibus dona-	Seminibus flore, flaminibus, &c petalis, donatæ. 1. Tuba destitutæ, Orchideæ; ut <i>Orchis</i> , <i>Hellebor</i> . 2. Liliacæ præditæ, ut <i>Gladiolus</i> , <i>Colchicum</i> . 3. Fructu intra florem, 1. Monopetalum; ut <i>Muscari</i> . 2. Hexapetalum; ut <i>Lilium</i> .	1. Plerumque multifidæ; ut <i>Veratrum</i> , <i>Butomus</i> . 2. Gymnopo- lypermæ, 1. Petalis circa ovarium ortis; ut <i>Adonis</i> , <i>Trinitas</i> . 2. <i>aphyllatæ</i> , ut <i>Ribes</i> , <i>Rosa</i> . 3. Pomiferae, 1. Umbilicatæ, ut <i>Ribes</i> , <i>Rosa</i> . 2. Non Umbilicatæ; ut <i>Prunus</i> , <i>Cerasus</i> . 4. Multiloculares; ut <i>Tilia</i> , <i>Helianthemum</i> . 5. Staminibus coalis- tis; ut <i>Malva</i> .
IV.	Monoco- ledones, Dicoryledones petalodæ.	1. Polyftemones, 2. Diploftemones, 3. Ifoftemones,	1. Vasculiferae; ut <i>Econymus</i> . 2. Flore fructu innato Gymno- difpermae; ut, <i>Umbelliferae</i> . 1. Dipetalo; ut <i>Gerania</i> . 2. Tetrapetaloidæ; ut, <i>As- fo</i> . 3. Quinquetido; ut <i>Opa- lus</i> . 4. Flore circa fructum pofti- to. <i>Cucurbitaceæ</i> ; ut <i>Bryonia</i> . 5. Solanaceæ; ut <i>Heckungi</i> , <i>Solanum</i> . 6. Alperifoliae; ut <i>Ichium</i> , <i>Symphytum</i> . 7. Dicarpeæ; ut <i>Astrepia</i> , <i>Peruvia</i> . 8. Hexapetalæ; ut <i>Hierba</i> . 4. Meioftemones; ut <i>Ligustrum</i> , <i>Verica</i> . 5. Staminibus ad pe ala feftuati- teris, 1. Siliqua breviori, ut <i>Uniloculares</i> . 2. longiori, ut <i>Pedicularis</i> . 6. Staminibus ad petala duplis feftuati- teris; ut <i>Pedicularis</i> . 1. Capsula uniloculari, ut <i>Oro-</i> Flore monopetalo, flaminibus quatuor inequalibus, 2. <i>banche</i> . 3. Biloculari; ut <i>Digitalis</i> . Floribus uni femini infidentibus congregatis; ut <i>Pappofæ</i> , <i>Capitatae</i> ; ut <i>Conjunctivæ</i> .



A Scale of Feet to be measured up.

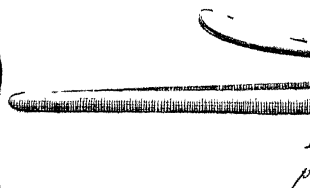
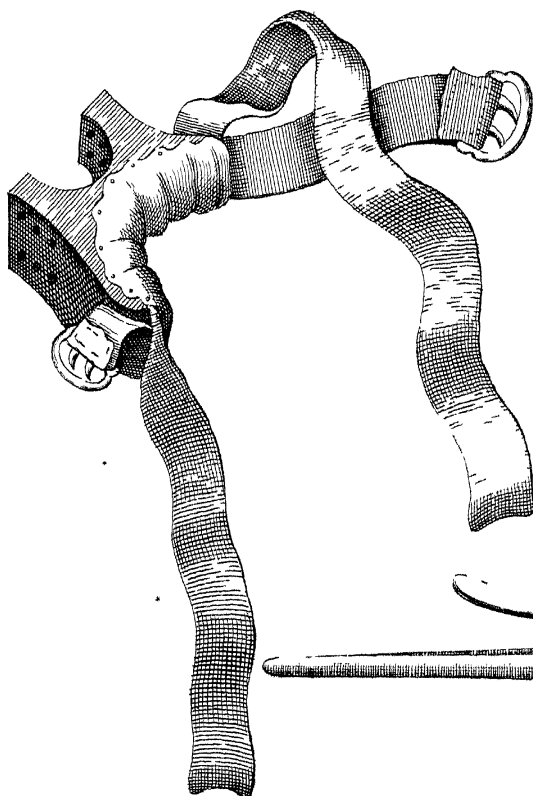


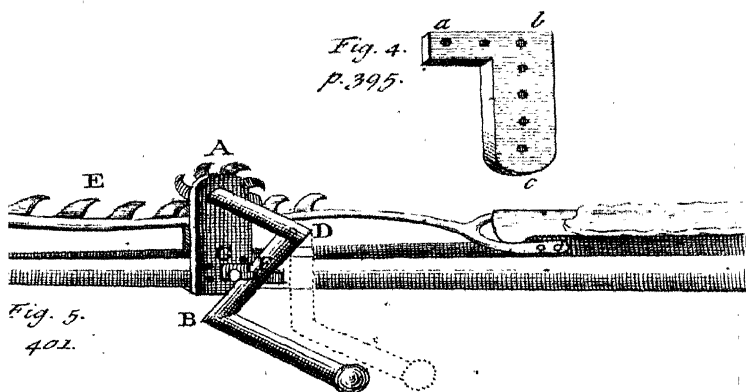
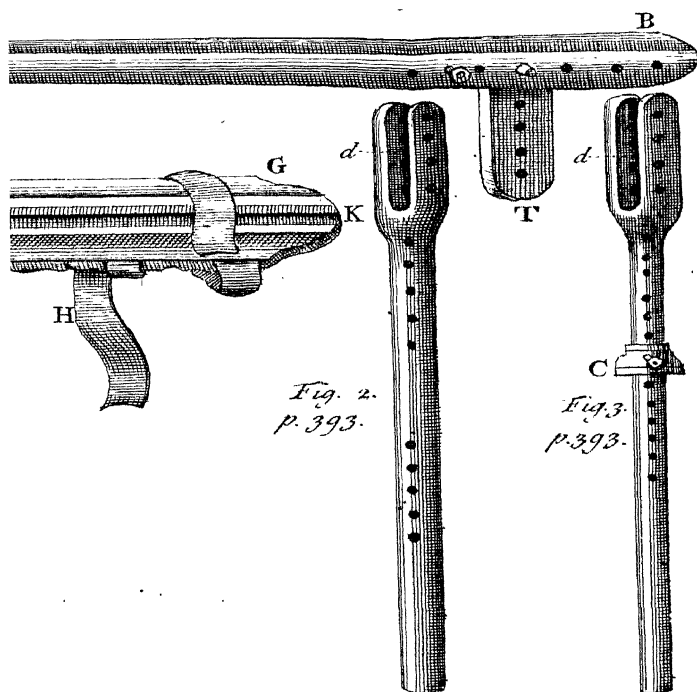
on the fore part of the Figure.



Philos Transact N^o 469 TAB.III.

Fig 1
p 392





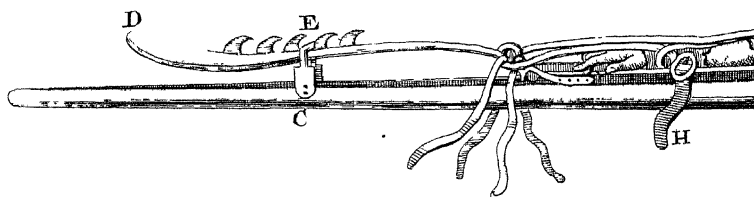


Fig. 2 p. 398.

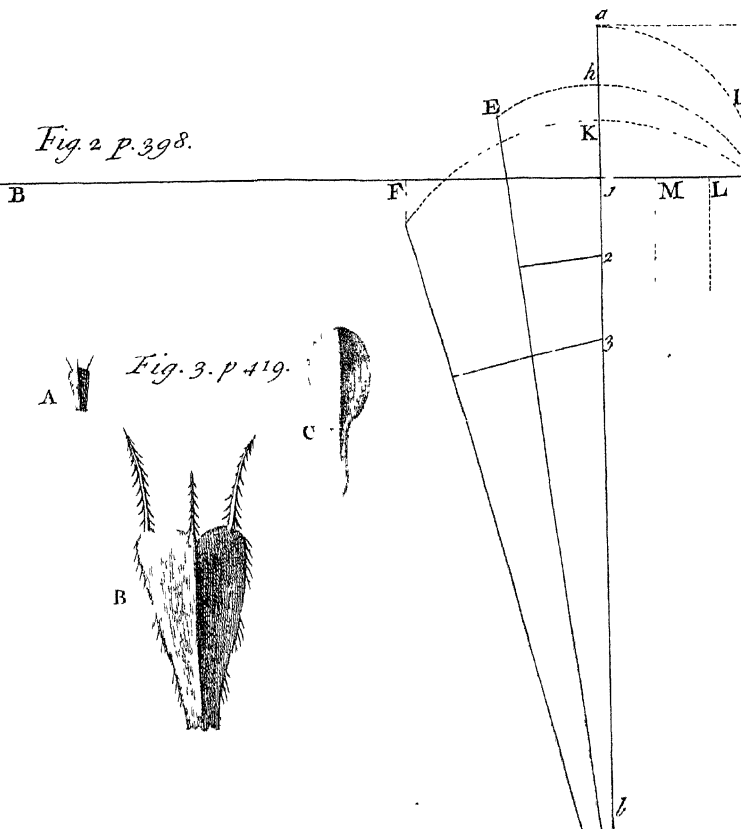
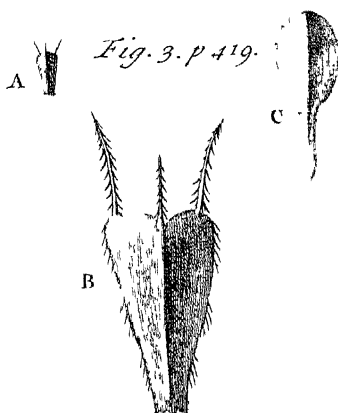


Fig. 3. p. 419.



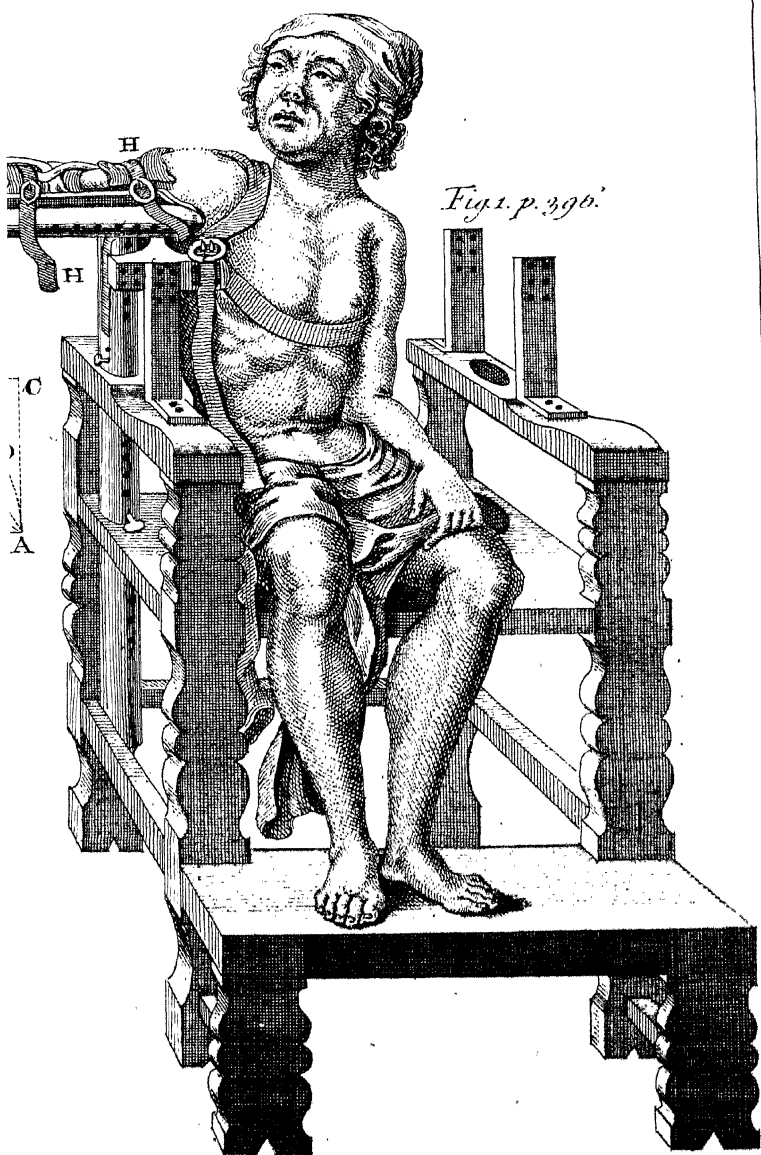


Fig. 1. p. 396.

Fig. 4.

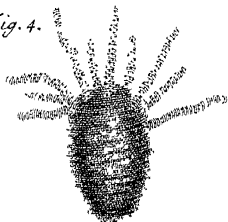


Fig. 5.



Fig. 6.

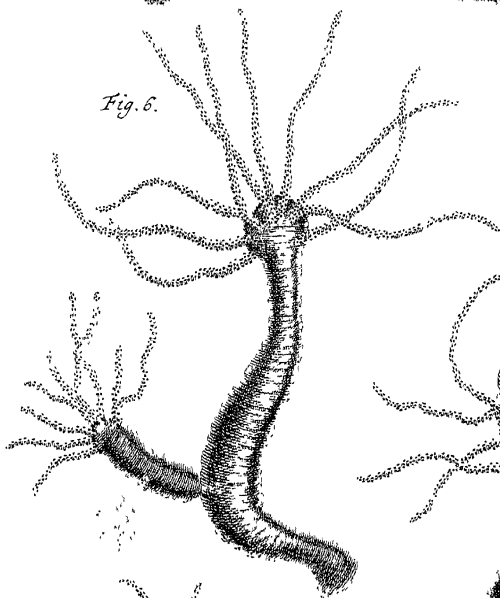


Fig. 7.



Fig. 9.

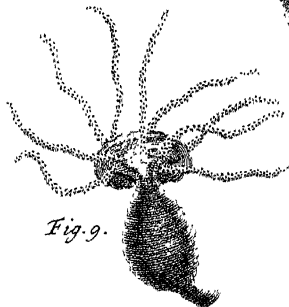


Fig. 3.



Fig. 1.

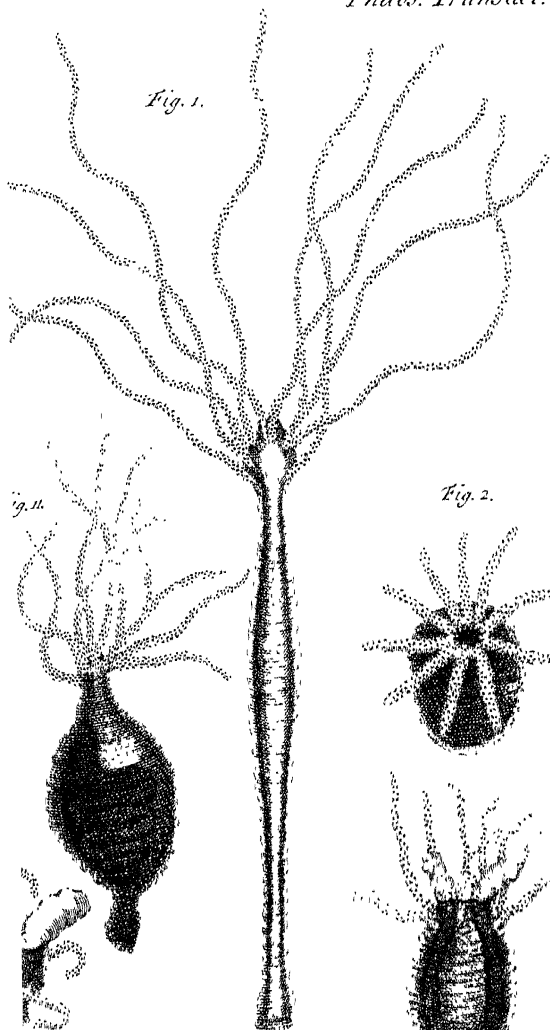


Fig. 11.

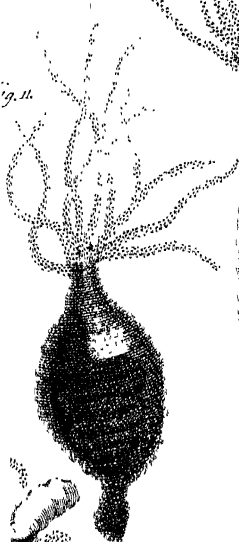


Fig. 2.

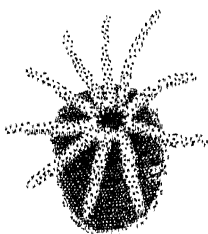


Fig. 12.

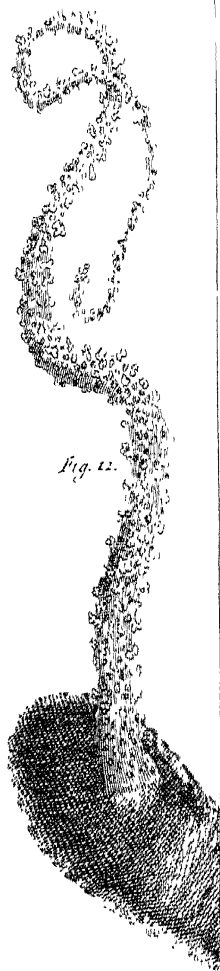


Fig. 8.

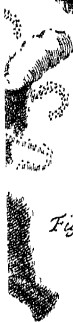
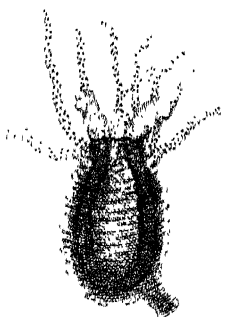
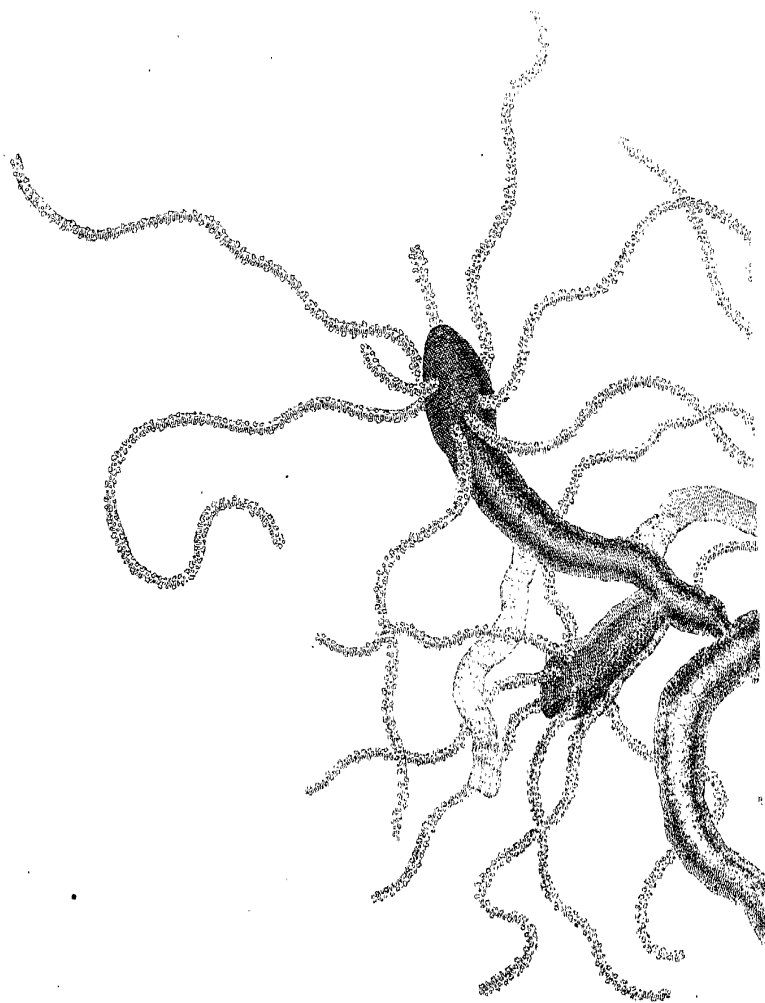
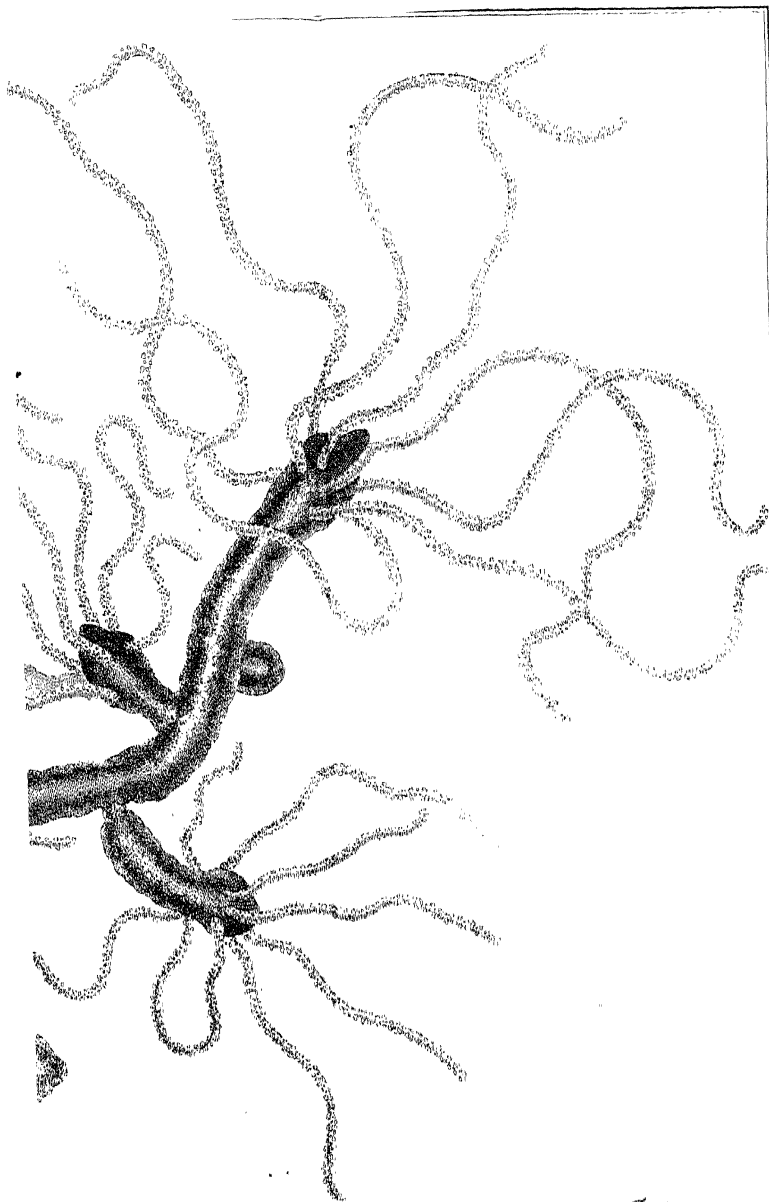


Fig. 10.







T. Myndus f.

PHILOSOPHICAL TRANSACTIONS.

From *February* 3. 1742-3. to *April* 21. 1743.

The CONTENTS.

- I. *Some Account of the Phoca, Vitulus marinus, or SEA-CALF, shewed at Charing-Crois, in February 1742-3. by Ja. Parsons, M. D. F. R. S. Page 383.*
- II. *The Ambe of Hippocrates for reducing Luxations of the Arm with the Shoulder, rectified; by M. le Cat, M. D. F. R. S. Surgeon to the Hôtel Dieu at Rouen, and Royal Demonstrator in Anatomy and Surgery: Extracted from the French by P. H. Z. F. R. S. 387.*
- III. *The Continuation of An Account of a Treatise of Fluxions, &c. Book II. by Colin Mc Laurin, Prof. Mathem. Edinburgh. F. R. S. 403.*
- IV. *Observations on the Mouth of the Eels in Vinegar, and also a strange aquatic Animal, sent in a Letter from the Rev^d Mr. Henry Milcs to Mr. Henry Baker, F. R. S. and by him communicated to the ROYAL SOCIETY: With a Drawing and Description of the said Animal, as viewed in the Microscope, by Mr. Baker. 416.*
- V. *Part of a Letter from Mr. Macky, Professor of History, to Mr. Mac Laurin, Professor of Mathematics*

The CONTENTS.

matics in the University of Edinburgh, and by him communicated to the President of the ROYAL SOCIETY; being an Extract from the Books of the Town-Council of Edinburgh, relating to a Disease there, supposed to be Venereal, in the Year 1497.
420.

VI. *Some Account of the Insect called the Fresh-water POLYPUS, before mentioned in these Transactions, as the same was delivered at a Meeting of the ROYAL SOCIETY, by the President, on Thursday, March 24. 1742-3.* 422.

VII. *An Account of a Book intituled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Investigation of the resisting Power of the Air to swift and slow Motions; by Benjamin Robins, F. R. S. as far as the same relates to the Force of Gunpowder.* 437.

Printed for T. WOODWARD, and C. DAVIS, over-
against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS
to the ROYAL SOCIETY. 1743.

E R R A T A.

Philosophical Transactions, N^o 468. p. 344. l. 16. *is* resolved into a Force parallel to the Base, *read*, resolved into a Force parallel to the Ordinates, and a Force parallel to the Base.

N^o 469. p. 424. l. 5. from the Bottom, *for* such *r.* every. p. 407. l. 3. *for* Rule *r.* Rules. p. 409. l. 4. *for* Divisors *r.* Denominators; l. 6. *for* or *r.* the; l. 7. *for* are *r.* being; l. 9. *for* Arcs *r.* Arcus.

I. *Some Account of the Phoca; Vitulus marinus; or SEA-CALF, shew'd at Charing-Cross, in Feb. 1742-3. by Ja. Parsons, M. D. F. R. S.*

*Read at a
Meeting of the
Royal Society,
on February
17. 1742-3.*

AS Authors have describ'd this Animal so largely already, I shall entertain this Honourable SOCIETY with only a few Remarks which occurred to me, upon taking as accurate a View as I could of it, which may conduce to furnish a truer Idea of the Creature than we now have from Authors.

The Figures given by *Aldrovandus*, *Johnston*, and others (being Profils) lead us into two Errors: 1st, They make a Cubit in the Fore-limb, which is not visible in any Shape, from the Surface of the Body; and, 2^{dly}, make the posterior Parts terminate in Two Fins, which on the contrary are actually webbed Feet (like those of Water-fowl) consisting of Five Toes, each having Three Articulations, and ending with Nails of a darkish Colour.

The Nails of the Fore-paws are very considerable, being like the Paws of a Mole, contriv'd for crawling upon Land, and partly for swimming, by a narrower Web between each Toe; but the hinder Feet are extensive Webs, serving alone to drive or row the Creature in the Waters.

— *Rondeletius*, as cited by *Gesner*, blames *Aristotle* for saying this Animal has Nails; which is strange, as that Historian is one of great Reputation; for it has very considerable ones.

The Animal, which was a Female, died Yesterday Morning, and the *Viscera* were as follows :

The Stomachs, Intestines, Bladder, Kidneys, Ureters, *Diaphragm*, Lungs, great Blood-vessels, and *Pudenda*, were like those of a Cow.

The Hairs of the Whiskers are very horny and clear.

The Spleen was Two Feet long, Four Inches broad, and very thin.

The Liver consisted of Six Lobes, each hanging as long and lank as the Spleen, with a very small Gall-bladder.

The Heart was long and flabby in its Contexture in general ; having a large *Foramen Ovale*, and very great *Columnæ carnosæ*.

In the lower Stomach were about Four Pounds Weight of flinty Pebbles, of which those I have the Honour to lay before you are Part ; all which are sharp and angular, as if the Animal chose them of that Form for cutting the Food. I believe this may be common to all the larger Sea-Animals, as they swallow many considerable Fishes whole, that after some Maceration in the first Stomach, they may be more easily ground small by these Pebbles in the other, for the Nourishment of the Creature.

The *Uterus* is of the horned Kind, each *Cornu* being considerably thicker than the Body or Duct leading to them : It is very fibrous, and the Fibres seem all longitudinal with the *Uterus* and *Cornua*, making ~~a~~conf-
lular Appearance. The *Ovaria* are very large, being granulated on the Surface with the *Ova*, under a very thin Membrane ; and the Opening into the Tubes lead-

leading to the *Cornua* is a great Hole. I have annexed a Drawing of this Part, as it is very Particular.

I refer the SOCIETY to the under-mentioned Authors for the other Properties of this Animal; such as their Love to their Young, their manner of Copulation, Inconstancies to their Females, Virtues in the Skin of preserving Persons from Thunder, who carry Part of it, as *Suetonius* relates of *Augustus Cæsar*, who dreaded it very much; and also of such Consent between the Skin of this Animal and the Sea, that although it be dried and kept in the most secret Place, whensoever the Sea is much disturbed, the Hairs rise up upon the Skin, and lie smooth when it is calm; with many other Particulars, which (if not fabulous) are very curious.

The Authors necessary to be read upon this Animal, are *Aristotle*, *Pliny*, *Aldrovandus*, *Rondeletius*, *Gesner*, *Wolfgangius*, *Johnston*.

As to the particular Figures of the Animal, that of *Aldrovandus* seems to have been taken from a stuffed Skin, having the hinder Feet like a Fish-Tail, and not at all like the Creature. *Rondeletius's* Figure has as little Truth as the former; and that given by *Gesner* in his Corollary on *Rondeletius*, is worse than any; having the Fore-parts upright like a *Sphinx*. This last Author has another Figure of the *Phoca*, which is rather like a *Lump-fish*, and almost triangular: These could never convey a just Idea of the Creature to such as delight in Natural History, which, I hope, I have made some amends for in my Figure annexed to this Account (see TAB. I.); having taken it from the living Animal with the utmost Care, and

which is thought perfectly like the Creature by all who have seen both.

The Animal is viviparous, and suckles its young by the *Mamillæ*, like Quadrupeds, and its Flesh is carnosous and muscular. This was very young, though Seven Feet and half in Length, having scarce any Teeth, and having Four Holes regularly placed about the Navel, as appears by the Figure, which in time become *Papillæ*.

See TAB. I.

Fig. 1.

Represents the Phoca lying upon the Right Side, that the Belly and Parts of Generation may be the better observed.

- A. *The Fore-feet and Breast.*
- B. *The Umbilicus and Holes of the Mammæ.*
- C. *The external Orifice of the Vagina, and the Anus.*
- D. *The hinder Feet, which are webbed.*
- E. *The Tail.*

Fig. 2.

Shews the Uterus taken out and extended.

- A. *The Body of the Uterus or Vagina.*
- B. *The Cornua Uteri.*
- C. *The Holes leading into the slender Tubes that end in the Extremities of the Cornua.*
- D. *The Ovaria.*
- E. *The Continuations of the Përitonæum.*

II. *The Ambe of Hippocrates for reducing Luxations of the Arm with the Shoulder, rectified; by M. le Cat, M. D. F. R. S. Surgeon to the Hôtel Dieu at Rouen, and Royal Demonstrator in Anatomy and Surgery: Extratted from the French by P. H. Z. F. R. S.*

Read Feb. 17.
1742-3.

THIS is one of those Chirurgical Operations which has many times puzzled the most skilful Surgeon. Among the Machines which Art has invented for the performing of it, *the Ambe of Hippocrates* is one of the most antient and most famous.

It is known to consist of an horizontal Lever *A*, and of a fixed Point *B*. (TAB. II. Fig. 2.) made of a Piece of Wood standing vertically, to the Extremity of which the Lever is joined by a Hinge. The Patient sitting, and his Arm, that is hurt, being raised, the Machine is pushed forward under the Arm-pit, so that the vertical Piece of Wood is applied along the Ribs, where the Lever enters into the Arm-pit up to the End of the luxated Bone, or even farther. This Circumstance is essential, and even recommended by *Hippocrates*: *Imprimis vero, says he, id elaborandum est, ut ligni summa pars, brachii capite superato quam penitissime alam subeat.* Sect. VI. p. 783. *Fæfir.* The Arm is tied to this horizontal Piece, and then an Assistant bears upon the *Scapula* and the *Clavicula*, as is seen in the Figures of *Scultetus*, Plate 21.

whilst

whilst another presses down the Lever, and thus makes the Bone come into its Place again.

Hippocrates, giving the Description of this Invention, and of its Use, acknowledges that this Method of reducing the Luxations of the Arm is incomparably better than all the others; for, says he, the working of it is sufficiently powerful; and provided Care be taken to push the Lever farther on under the Arm-pit than where the Bone of the Arm lies, the Extensions and Counter-extensions are equal, and the Bone of the Arm is safe: He adds, That by this Method fresh Luxations are reduced sooner than one thinks, and even before the Extension appears to have been made; and that, as for old Luxations, they can only be reduced by this Method; unless, by their being too old, the Cavity of the Articulation be filled up, and that the Head of the Bone has formed to itself an Articulation in the Place where it fell: He even believes, that such a Luxation may be reduced; for, says he, What is there that cannot be moved by sufficient Forces? But at the same time he thinks that the reduced Bone will not remain in its Place, but luxate itself again, and fall back into the new-formed Articulation, which it has formed to itself.

(Here follow Hippocrates's Words in Latin.)

“ Hæc reponendi humeri ratio longe optima censetur. Justissimam enim molitionem facit, si vel
 “ solum intro magis quàm brachii caput lignum
 “ immittatur, justissimæque sunt in utramque partem
 “ librationes, & ossi brachii securitatem præstant.
 “ Recentia igitur opinione citius reconduntur, ac
 “ pri-

“ priusquam extensio facta videatur. Quinetiam ubi
 “ inveteraverint, hæc sola repositio restituere potest,
 “ nisi jam temporis lapsu caro quidem articuli cavi-
 “ tatem occupaverit, & brachii caput locum in quem
 “ declinavit, consuetudine tritum jam sibi fecerit;
 “ enimvero tamen ita inveteratum brachii luxum
 “ reponere mihi posse videtur. Quid non enim iusta
 “ molitio moveat? Non tamen loco manere posse
 “ existimo, sed, ut consuevit, prolapsurum.” *Loco
 citato.*

One of the greatest Surgeons of our Age (M. *Petit*) in his Treatise of the Diseases of the Bones, was sensible of all the Perfections of the *Ambe* of *Hippocrates*: He acknowledges with that Father of Surgery, that this Machine has a *sufficient Force*, and is more than sufficient——that it makes an *Extension and a Counter-extension equally strong*——He even adds——that *the Arm is placed there as it ought to be, in order to relax the Muscles*, or at least *stretch them equally*, which is the Fourth Rule the Author proposes to be observed, *p. 42.* in making the Extension and Counter-extension. But at the same time M. *Petit* does not dissemble some essential Defects he finds in this Invention, and which, without doubt, were unknown to *Hippocrates*.

The capital Defect in this *Ambe* is, that *it pushes the Head of the Bone into its Cavity, before the Extension and Counter-extension are made.* The dangerous Consequences of this Defect, are, according to M. *Petit*, 1st, That the Reduction is very difficult, because the Bone is not conducted by the same Way it took in luxating itself, and that one meets with Obstacles from the Parts that surround it,
 even

even the *Scapula* itself, on which it articulates. 2dly, In making those Efforts for surmounting those Obstacles, one runs the Risque of turning inwards the cartilaginous Edge of the Cavity of the *Scapula*, or the *Capsula ligamentosa*. The Second Defect of the *Ambe* of *Hippocrates* is, that it cannot move the luxated Bone but from below upwards; consequently, this Machine is only proper in Luxations directly downwards; and yet it is certain, that the Arm luxates itself both outwards and inwards; and even it is known to all Practitioners, that Luxations forward are very frequent. Here you have a great Number of Luxations of the Arm, where the *Ambe* becomes useless: Now, if the *Ambe* of *Hippocrates* is useless in all Luxations outwards, and in Luxations inwards, which are very frequent, if it is dangerous in Luxations downwards, the only ones it is fit for, one must own, that this Machine, so much cried up by *Hippocrates*, is yet very imperfect.

These Imperfections are real ones, but the Advantages, which one cannot but own it has, are so constant, and so superior to those of any other Practice, that one naturally inclines not to part with it, but becomes desirous to remove those Defects it has, without which it would certainly be, as *Hippocrates* assures, the most perfect of all Machines made use of in reducing a luxated Arm: For supposing an *Ambe*, which makes a sufficient *Extension* and *Counter-extension*, before it leads the Bone into the Cavity, or at the same time it does so, and which also might lead it from the Right to the Left, and from the Left to the Right, as well as from below upwards, it is certain there can be no Method to be compared to this;

this; because there is none in which concur at once so much Force and Expedition, joined to such Simplicity, Regularity, and Safety, that are quite singular. For that Method in which a Surgeon only employs his own Strength, and that of his Assistants, is commonly insufficient; and the other, in which he helps himself with the Pulley, is perplexed with a great *Apparatus*, is long, and still very much wants the Hands of the Surgeon, and of his Assistants: All which are Circumstances which render the Method more complicated, and less sure.

These are the Motives that have engaged me to contrive the new *Ambe* I herewith have the Honour to lay before the SOCIETY, in which I have endeavoured to rectify all the Defects before-mentioned.

A Description of the new Ambe.

TAB. II. The Basis of the whole Machine is an Elbow-chair all of solid Wood, higher than others usually are, in order to give Room to the Lever to play the more freely, which cannot be lowered any farther than to the Floor on which the Elbow-chair stands: To prevent any Uneasiness to the Patient from that Height of the Chair, it has a Foot-stool that makes Part of the Chair, and brings the Seat to its usual Height.

Each Arm of the Chair is pierced with a round Hole, to receive the *Stem* or *Foot* of the *Ambe*. If the Luxation is on the Right Side, the Foot is run through on the same Side, and *vice versa*. The Patient is tied partly to the Back of the Chair, partly to a Piece joined to the Chair on that Side where

the *Ambe* is placed. This solid Union of all the Pieces of the Machine between themselves, and with regard to the Patient, furnish its Action with all the Force and Certainty possible. The *Ambe* of *Hippocrates* can play but to a small Extent: It is separate from the Chair in which the Patient sits, and he is left to the Care of the Assistants; all disadvantageous Circumstances, which are remedied by my Machine.

In that of *Hippocrates*, the Body of the Patient has no other Support against the Extension of the Lever than the very vertical Piece (see TAB. II. Fig. 2. *B.*) on which the Lever rests; this Piece is narrow, has no Proportion, or, if one may say so, no Union with the Figure of the Body to which it is applied, and consequently must change his Position on that Piece upon the least Effort the Patient makes.

The Foot of my Lever has no Connexion with the Patient's Body: There is between the Foot and his Body a particular Piece which I call *the Bodice*, represented in Fig. 1. TAB. III. One will see there, that it is made to fit itself to the Body; and, in order to render that Application easy, that Part which touches the Body, is quilted. This Bodice is fixed to the Arm of the Chair between Two large Iron Checks, *a, b*, Fig. 1. TAB. II. by Two strong Iron Pins, which run through them, and are stopped at their Extremities with Nutskewed on. The concave Part of this Piece, where the Body enters, is placed perpendicularly under the End of the Lever, however so that the Lever be a little farther advanced towards the Patient, than the Bottom of the Bodice, to the end that the Lever may thrust itself the better in under the Arm-pit. As there are Cases where the Head of the Lever ought
to

to be very short, or very near the Point it rests upon, and others again on the contrary, where that Extremity of the Lever ought to be longer, and farther off the Point of its Rest, the Bodice of course ought to be set backward or forward, as the End of the Lever is, the Direction of which it follows every-where. For this Reason we have contrived Two Rows of Holes along the Sides of the Bodice, and between these Two Sides we got a Notch cut out, to make room not only for the Foot, or for the Point it rests upon, which may meet there, but also for a Part of the Lever, which I call its Spur, which always moves towards that Notch when the Lever is lowered. The Figures and the Use of the Machine will shew the Necessity of this Construction much better than any Description. From the said Bodice come out Two broad Straps of the strongest Leather with their Buckles. One of those Straps is to go about the Back of the Chair, and round the Body of the Patient; the other goes over the Shoulder, very near the Articulation, and keeps the *Scapula* and the *Clavicula* in their Situation against the Efforts of the Lever; see TAB. II. and IV.

That Part of my Machine, that may be called the *Ambe* properly said, is composed, like that of *Hippocrates*, of Two Pieces; one vertical, which I call the Foot of the *Ambe*; and the other horizontal, which forms the Lever. It is chiefly in these Two Pieces that my *Ambe* differs from that of *Hippocrates*.

The Foot is a Piece made either of Wood, TAB. III. Fig. 2. or of Iron, Fig. 3. Its upper Extremity is split into a sort of Mortise, which receives the Spur

or Tenant *T* of the Lever *A, B*. It is pierced by several Holes, which answer to as many others on the Spur. Below this Mortise the Foot becomes more slender and cylindrical; by this Part it enters into a round Hole in the Arm of the Chair; this slender Part of the Foot is pierced by several Holes, in order to run an Iron Pin through, which lies flat on the Arm of the Chair, and keeps the Foot raised to a Height proper for the Person that undergoes the Operation: For the greater Security one may run Two Pins through; one which rests upon the Arm of the Chair, and the other on the Seat itself, through which the Foot passes also. The Iron Foot, Fig. 3. may be provided with a sort of large Ring *C*, under the Pin, which will render its Rotation the easier. If one should prefer an Iron Foot, one may easily judge, that the Hole for it in the Arm of the Chair must be made narrower, either by filling up the old one with an Iron Box or Clout, which may be taken away, if one will use a wooden Foot; or one may even at first fit those Holes for the Iron Foot, setting the wooden one quite aside.

The Lever *A, B, H, B*, TAB. II. Fig. 1. is the most compound Piece of all, and withal the most important. It is made of a real Lever *A, B*, and of a Piece fitted to it *D, G*, TAB. II. and III. The Lever properly so called *A, B*, TAB. II. is made round on its inferior Surface; the upper Surface is flat, and all along on the Middle of it there runs a Rod, forked at the End, which fits to a Groove of the same Figure in the inferior Surface of the Sliding-piece *F, G*, TAB. III. This Lever grows less and less towards the Extremity *A*, where the moving Power is to be applied; the other Extremity,
B,

B, is somewhat rounded off at its End, in order to insinuate itself the better under the Arm-pit. On this bigger Extremity is a sort of *Spur* or *Tenant*, *T*, TAB. III. the upper Part of which is joined to the Lever by Two Iron Pins, so that, upon taking out the Pins, the Spur comes out, and separates itself from the Lever, as appears by Fig. 4. TAB. III. It was necessary to make this Spur moveable, and give it the Figure of a square Rule in which it appears, in order to bring it quite close to the End of the Lever, or set it back, according as it may be necessary. For this Reason the upper Part of this Spur *a, b*, slides along in a Mortise or Groove of the Length of one Foot contrived under the Lever, beginning from its Extremity *B*, to which answers the Shoulder *b*, of the Spur.

The rest of the Tenant, or its principal Part *c*, is fitted to enter into the Mortise *d*, which is the uppermost Part of the Foot, Fig. 2, 3. TAB. III. They are both of them pierced with a Row of Holes, through one of which one must run an Iron Pin, to unite them, and to form the Point of Rest, or the Hinge of the Lever. Towards the other Extremity *A* of the Lever, there is a Piece of Iron *C*, made Arch-wise, under which passes the elastic Tail *D, f*, of the Rod fastened to the Sliding-piece *F, G*, and into which catch Teeth made on the said Tail, as is seen in TAB. III. and IV. This Iron Arch ought to be very solid, because it keeps down the Arm, and supports all the Effort of the Lever. I will give to the Sliding-piece *F, G*, which is fitted to the Lever, the Name of the *Bracer*; it is a Groove made of one Piece of Wood, represented in its Situation in TAB. II. and IV. This Piece is hollow in the upper Surface, as is just now said, to
place

place the luxated Arm into; this Cavity is quilted, and has Three Girts *H*, with Buckles, to tie the Arm fast and conveniently; they are made of strong Leather. It has on its inferior Surface a Groove with a Dove-tail *K, K*, TAB. III. to lay hold of the Rod of the Lever, and to slide in it without being separated from it, unless it be in sliding beyond the Extremity *B*, of the Lever, where it pulls out like a Drawer, which is easily done, if the *Bracer* has nothing to stop it upon the Lever. The Extremity of the *Bracer*, which answers to the thick End of the Lever, is rounded, in order to enter jointly with it under the Arm-pit; the other gives hold to the Piece of Iron *D, E*, which I called above by the Name of *the elastic Tail of the Bracer*. This latter consists of Four Parts; the Fork *F*, which attaches itself to the inferior lateral Surfaces of the Bracer; the Spring *f*, which is the Piece that follows next, the longest and slenderest of all; the Teeth *E*, and the Handle *D*.

The Use of the new Ambe.

The Patient, being undressed down to the Waist, is placed in the Arm-Chair, as in TAB. IV. Fig. 1. Next, the Lever, furnished with its Bracer, is raised and kept in an horizontal Position, taking great Care, as *Hippocrates* recommends, to push this Bracer as far as may be under the Arm-pit to the End of the Bone of the Arm, and even beyond, if possible, to the end that the *Humerus*, supported by the Bracer in all its Length, may be secure against all the Power of this Machine, and that its Violence may only act upon those Muscles which keep this Bone out of its Place.

Besides

Besides the Quilting, which the Bracer is lined with, a small Cushion is put upon its Extremity, in order to lodge still more conveniently the Head and the Neck of the *Humerus*, and to preserve the soft Parts from any Contusion, which the Impulse of the Machine might produce, by its greatest Forces acting upon that Part.

The Arm being thus placed and well stretched out upon the Bracer, you tie about it Two Sliding-knots, one above the Elbow, and the other over the Wrist, after having guarded those Parts with a very thick and soft Compress; the Two Sliding-knots are fastened to the Fork of the elastic Tail of the Bracer; after which you complete the fixing of the Arm with the Three Girts of the Bracer, under which are also put Compresses like those just mentioned.

The Arm being thus well adjusted, you endeavour to give to the Body and to the Hollow of the Articulation of the luxated Bone the proper Situation and Steadiness necessary for the Success of the Operation, which is easily executed with this Machine, by the Girts of the Bodice, of which the horizontal one keeps the Patient's Breast closely applied against this Piece, and the vertical Girt retains the *Scapula*, the *Clavicula*, in short, all the Parts where the Bone is to be pushed back, in a Situation proper for receiving it, and for not deviating by yielding to the Efforts of the Machine.

Every thing being thus disposed, the Surgeon places himself behind the Patient, mounted upon something that raises him high enough to inspect the Effects of the Process; to examine by the Touch where it operates; in short, to conduct the Whole by Feeling and by

by the Eye. The Surgeon being placed, the Assistant who is to conduct the Extremity of the Lever, works it according to his Directions, but perfectly slowly, that the Extensions may be made with less Pain, and more effectually.

If the Luxation is below, it is sufficient for its Reduction to lower the Extremity of the Lever, as is done with the *Ambe* of *Hippocrates*. But here appears a great Difference between the working or playing of these Two Sorts of Levers. The *Ambe* of *Hippocrates* is a plain Lever *A, B*, Fig. 2. TAB. IV. the Motion of which is from *A* to *a*, and consequently has for its Extension only the Space *C, a*, when it is brought to its last Term of becoming perpendicular, *a, b*, whilst it has all *A, C*, or *1, a*, for its Elevation. The *Ambe* of *Hippocrates* therefore almost only raises the Bone of the Arm, without scarcely stretching it; and this is the Defect, which *M. Petit* with Reason blames it for; and which is still more sensible, if one takes the Action of the Lever in *D*, the Point whereabouts it must meet the Edge of the Cavity, and may cause those Mischiefs that are apprehended from it; but instead of placing the fixed Point of that Lever in *1*, lower it to *2*, by the means of the Tenant *1, 2*; then the Direction of the End of the Lever becomes *A, E*; its Elevation is but *1, b*; and the Extension it produces is *A, E*, or *D, E*: If you lower still the Lever's Point of Rest, as in *3*, by a longer Spur, the Elevation of its Extremity is reduced to *1, k*; and the Extension it produces, reaches from *A* to *F*, if one carries those Levers as far as they will go, which is never necessary. In short, it will be in your Power to give to this Lever an Extension as great
as

as you please, joined to a very small Elevation : To this End you need only set backwarder the Lever's Point of Rest, along the Perpendicular marked in Fig. 2. TAB. IV. Now this is precisely what the Spur does, which we have added to our *Ambe*; the Holes it is pierced with, as well as the Mortise of the Foot, are placed in different Degrees, as the Points 1, 2, 3 ; and these Holes, as has been said, are the Places of the Pin which forms the Lever's Hinge or Point of Rest.

The Gradation of those Holes therefore enables you to augment at Will *the Extension*, whilst the Elevation diminishes in the same Proportion ; but if you have a mind the Elevation should diminish more or less than in the foresaid Proportion, for Instance, you want to make a great Extension, and a very small Elevation, there is nothing easier for it than our Machine. You need only push the Spur 1, 3, which is moveable, as you know, towards the End of the Lever to *L*, and stop it there : Then the End of the Lever *A, L*, being very short, it has but little room to play ; on the contrary, if you will have a great Elevation, you need only bring back the said Spur to *M*, or 1, or still farther ; the farther you remove from the End of the Lever, the more it will have room to play, and the more considerable will be its Elevation. It is true, the Power of the Lever will decrease in the same Proportion ; but this Power is so great, that Losses like this ought to be reckoned for nothing.

You have it therefore in your Power with this sort of *Ambe* to make, as Occasion requires, such Extensions and Counter-extensions as you please ; and you may likewise vary all the Degrees of the Elevation, which shall be necessary to give to the Bone that is

to be reduced; and these are the Perfections which have been hitherto required in this Machine.

Commonly, when the Bone of the Arm is sufficiently stretched and raised, so as to be on a Level with the Cavity of the Articulation, those Bones replace themselves as it were of themselves, because this Level is not always exact; on the contrary, the Extension and Counter-extension being never regular enough to hinder the *Scapula*, which is a moveable Part, from following a little the Head of the Bone, or its Extension, it happens almost always, that this Head bears pretty strongly against the Edge of the Cavity, and consequently does not fail to fall into the said Cavity, as soon as it has only passed its Edge, and even before it has met the Level, or the Axis of the Hollow of the Articulation; but it is otherwise after an Extension, a Counter-extension, and an Elevation so regular as those which may be performed by our Machine; it may happen, that after the Three preceding Operations, the Head of the Bone, without having touched the Edge of the Cavity, will be placed over-against this Cavity, and upon a Level with its Axis, without being able to enter into it, by reason of the Firmness and Exactness of the Powers for retaining the opposite Parts in this State of regular Extension; and, in this Case, there will remain for you, in order to finish the Operation, to conduct the Head of the Bone into its Cavity, or to let it go into it: But what will you do then? If you slacken the Extremity of the Lever, or if you lift the same up, you will bring the Head back to the same Place where you took it up; that is to say, you will bring the Luxation to its former State. If you resolve to
relax

relax the running Knots, the Operation will be long, and your Patient will have time enough to cry out.

In order to avoid these Inconvenients, I mounted the Bracer on the Lever in a Groove, and I stopped it in this State by the Teeth of its elastic Tail ; by the means of this Construction, when the Surgeon perceives, that the Bone is over-against its Cavity, he directs the Assistant who attends the Extremity of the Lever, to press upon the Handle (TAB. IV. Fig. 1.) *D* of the elastic Tail of the Bracer, to the end that the Teeth placed under the Arch *C*, near the said Handle, may quit their Hold, and that the whole Bracer, which is now no longer stopped, may slide on the Lever towards the Patient, and by this means let the Head of the Bone enter into its Cavity.

The Necessity of this Management with our *Ambe* is a Demonstration, that it is far from having that capital Fault with which M. *Petit* reproaches the *Ambe* of *Hippocrates*: viz. " That it pushes the " Head of the Bone into its Cavity, before the Extension and Counter-extension are made." I hope the Machines, whereby I have prevented this Fault, and have procured to my *Ambe* the opposite Perfections, will appear sufficiently simple.

If any body should be apprehensive, that the re-entering of the Head of the Bone might be too sudden, and occasion a Shock that might hurt the said Bones, it will be easy to remedy against it, by substituting to the Stop, into which catch the Teeth of the Bracer, a toothed Wheel *A*, (Fig. 5. TAB. III.) having in its Centre a Handle *B*, *D*; which Handle during the Operation will be stopped by the Piece of Iron *C*, fixed upon this Piece by the Skrew *F*; the said Handle

will also stop the Teeth *E*, which catch into the tooth'd Wheel; and when the Bracer is to be loosened, the Assistant, who holds the Lever with one Hand, will take the Handle with the other, and having got the Skrew *F* taken off, he will remove from the Piece *C*, that stops it, the Part *D*, *B*, of the Handle, by the means of its moveable Arbor *D*, so that the Handle will come at a Right Angle, as it is represented by Dots: Then the Assistant's Hand, sustaining all the Effort of the Handle and of the Bracer, will moderate by the Handle the sliding of the Bracer, and the entering of the Head of the Bone into its Cavity, with all the Slowness he shall think proper for this Operation.

Thus much concerning the Reduction of a Luxation of the Arm below; it is known, that this is the only sort of Luxation in which the *Ambe* of *Hippocrates* can be made use of (the second Defect observed by *M. Petit* in this Machine). I have succeeded in remedying against this Defect by the simplest thing in the World, *viz.* by giving to the Foot that enters into the Arm of the Chair a cylindrical Shape, by which means it is able to turn all manner of ways; so that if the Luxation is forwards, one only needs turn the Extremity of the Lever accordingly, lowering it at the same time enough to make the necessary Extension and Elevation; by this Turn of the Extremity of the Lever forwards, the Head of the Bone is of necessity carried backwards, and replaced into its Cavity. One easily conceives, that one must go to work in the opposite way, when the Luxation is backwards, and so on as for the rest; all according to the Directions of the Surgeon placed at the Articulation, who is to be attentive to examine the State
of

of the Parts, and to order in what Direction and how much is necessary to be done.

III. *The Continuation of An Account of a Treatise of Fluxions, &c.* Book II. by Colin Mc Laurin, *Prof. Mathem.* Edinburgh. F. R. S. *

Presented March
10. 1742-3.

IN the first Book, the Author described the Method of Fluxions, and its Application to Problems of different Kinds, without making use of any particular Signs or Characters, by geometrical Demonstrations, that its Evidence might appear in the most simple and plain Form. In the second Book, he treats of the Method of Computation, or the Algebraic Part; to the Facility, Conciseness, and great Extent of which, the Improvements that have been made by this Method are in great measure to be ascribed. In order to obtain those Advantages, it was necessary to admit various Symbols into the Algebra: But the Number and Complication of those Signs must occasion some Obscurity in this Art, unless Care be taken to define their Use and Import clearly, with the Nature of the several Operations. An Example of this is given by an Illustration of one of the first Rules in Algebra. As it is the Nature of Quantity to be capable of Augmentation and Diminution, so Addition and Subtraction are the primary Operations in the Sciences that treat of it. The positive Sign implies an Incre-

* See the Beginning of this Account, N^o 468. p. 325.

ment,

ment, or a Quantity to be added. The negative Sign implies a Decrement, or Quantity to be subtracted : And these serve to keep in our View what Elements enter into the Composition of Quantities, and in which manner, whether as Increments or Decrements. It is the same thing to subtract a Decrement as to add an equal Increment. As the Multiplication of a Quantity by a positive Number implies a repeated Addition of the Quantity, so the Multiplication by a negative Number implies a repeated Subtraction : And hence to multiply a negative Quantity, or Decrement, by a negative Number, is to subtract the Decrement as often as there are Units in this Number, and therefore is equivalent to adding the equal Increment the same Number of Times ; or, when a negative Quantity is multiplied by a negative Number, the Product is positive. When we inquire into the Proportion of Lines in Geometry, we have no regard to their Position or Form ; and there is no ground for imagining any other Proportion betwixt a positive and negative Quantity in Algebra, or betwixt an Increment and a Decrement, than that of the absolute Quantities or Numbers themselves. The Algebraic Expressions, however, are chiefly useful, as they serve to represent the Effects of the Operations ; and such Expressions are not to be supposed equal that involve equal Quantities, unless the Operations denoted by the Signs are the same, or have the same Effect. Nor is such Expression to be supposed to represent a certain Quantity ; for if the $\sqrt{-1}$ should be said to represent a certain Quantity, it must be allowed to be imaginary, and yet to have a real Square ; a way of speaking which it is better to avoid. It denotes only, -
that

that an Operation is supposed to be performed on the Quantity that is under the radical Sign. The Operation is indeed in this Case imaginary, or cannot succeed; but the Quantity that is under the radical Sign, is not less real on that Account. The Author mentions those things briefly, because they belong rather to a Treatise of Algebra than of Fluxions, wherein the common Algebra is admitted.

In order to avoid the frequent Repetition of figurative Expressions in the Algebraic Part, the Fluxions of Quantities are here defined to be any Measures of their respective Rates of Increase or Decrease, while they are supposed to vary (or flow) together. These may be determined by comparing the Velocities of Points that always describe Lines proportional to the Quantities, as in the First Book; but they may be likewise determined, without having recourse to such Suppositions, by a just Reasoning from the simultaneous Increments or Decrements themselves. While the Quantity A increases by Differences equal to a , $2A$ increases by Differences equal to $2a$, and (supposing m and n to be invariable) $\frac{mA}{n}$ increases by Differences equal to $\frac{ma}{n}$, and therefore at a greater or less Rate than a , in proportion as m is greater or less than n . Thus a Quantity may be always assigned that shall increase at a greater or less Rate than A , (*i.e.* shall have its Fluxion greater or less than the Fluxion of A) in any Proportion; and a Scale of Fluxions may be easily conceived, by which the Fluxions of any other Quantities of the same kind may be measured.

Let B be any other Quantity whose relation to A can be expressed by any Algebraic Form; and while A increases by equal successive Differences, suppose B to increase by Differences that are always varying. In this Case, B cannot be supposed to increase at any one constant Rate; but it is evident, that if B increase by Differences that are always greater than the equal successive Differences by which $\frac{mA}{n}$ increases at the same time, then B cannot be said to increase at a less Rate than $\frac{mA}{n}$; or if the Fluxion of A be represented by a , the Fluxion of B cannot be less than $\frac{ma}{n}$. And if the successive Differences of B be always less than those of $\frac{mA}{n}$, then surely B cannot be said to increase at a greater Rate than $\frac{mA}{n}$; or the Fluxion of B cannot be said to be greater in this Case than $\frac{ma}{n}$.

From those Principles the primary Propositions in the Method of Fluxions, and the Rules of the direct Method, with the fundamental Rules of the inverse Method, are demonstrated. We must be brief in our Account of the Remainder of this Book. The Rule for finding the Fluxion of a Power is not deduced, as usually, from the Binomial Theorem, but from one that admits of a much easier Demonstration from the first Algebraic Elements, *viz.* That when n is any integer positive Number, if the Terms E^{n-1} , $E^{n-2}F$, $E^{n-3}F^2$, $E^{n-4}F^3$, F^n , (wherein the Index of E constantly decreases, and that of F increases by the same Difference Unit) be multiplied by $E - F$, the Sum of the Products is $E^n - F^n$; from which it is obvious, that when E is greater than F ,
then

then $E^n - F^n$ is less than $nE^{n-1} \times \overline{E - F}$ but greater than $nF^{n-1} \times \overline{E - F}$.

The Ruels are sometimes proposed in a Form somewhat different from the usual manner of describing them, with a View to facilitate the Computations both in the direct and inverse Method. Thus, when a Fraction is proposed, and the Numerator and Denominator are resolved into any Factors, it is demonstrated, that the Fluxion of the Fraction divided by the Fraction is equal to the Sum of the Quotients, when the Fluxion of each Factor of the Numerator is divided by the Factor itself, diminished by the Quotients that arise by dividing in like manner the Fluxion of each Factor of the Denominator by the Factor.

The Notation of Fluxions is described in Chap. 2. with the Rules of the direct Method, and the fundamental Rules of the inverse Method. The latter are comprehended in Seven Propositions, Six of which relate to Fluents that are assignable in finite Algebraic Terms, and the Seventh to such as are assigned by infinite Series. It is in this Place the Author treats of the Binomial and Multinomial Theorems (because of their Use on this Occasion), and they are investigated by the direct Method of Fluxions. The same Method is applied for demonstrating other Theorems, by which an Ordinate of a Figure being given, and its Fluxions determined, any other Ordinate and *Area* of the Figure may be computed. The most useful Examples are described in this Chapter, by computing the Series's that serve for determining the Arc from its Sine or Tangent, and the

Logarithm from its Number, and conversely the Sine, Tangent, or Secant, from the Arc, and the Number from its Logarithm.

The inverse Method is prosecuted farther in the Third Chapter, by reducing Fluents to others of a more simple Form, when they are not assignable by a finite Number of Algebraic Terms. When a Fluent can be assigned by the Quadrature of the Conic Sections, (and consequently by circular Arcs or Logarithms) this is considered as the second Degree of Resolution; and this Subject is treated at Length. An Illustration is premised of the Analogy betwixt Elliptic and Hyperbolic Sectors formed by Rays drawn from the Centres of the Figures: The Properties of the latter are sometimes more easily discovered because of their Relation to Logarithms, and lead us in a brief manner to the analogous Properties of Elliptic Sectors, and particularly to some general Theorems concerning the Multiplication and Division of circular Sectors or Arcs. When Two Points are assumed in an Hyperbola, and also in an Ellipsis, so that the Sectors terminated by the Semi-axis; and the Two Semi-diameters, belonging to those Points, are in the same given *Ratio* in both Figures, then the Relation betwixt the Semi-axis and the Two Ordinates drawn from those Points to the other Axis, is always defined by the same, or by a similar Equation in both Figures. This Proposition serves for demonstrating Mr. Cotes's celebrated Theorem, as it is extended by M. *De Moivre*, by which a Binomial or Trinomial is resolved into its quadratic Divisors, and various Fluents are reduced to circular Arcs and Logarithms. The Demonstrations are also rendered more easy of
the

the Theorems concerning the Resolution of a Fraction, that has a multinomial Denominator, into Fractions that have the simple or quadratic Divisors of the Multinomial for their several Divisors. These Demonstrations are derived from the Method of Fluxions itself, without any foreign Aid; or invariable Coefficients are determined by supposing the variable Quantity or its Fluxions to vanish.

When a Fluent cannot be assigned by the Arcs of Conic Sections, it may however be measured by their Arcs in some Cases; and this may be considered as the Third Degree of Resolution, or the Fluents may be called of the Third Order. On this Occasion some Fluents are found to depend on the Rectification of the Hyperbola and Ellipsis, which have been formerly esteemed of an higher kind. The Construction of the elastic Curve, with its Rectification, and the Measure of the Time of Descent in an Arch of a Circle, are derived from Hyperbolic and Elliptic Arcs; and the Fluents of this kind are compared with those of the First or Second Order by infinite Series. Because there are Fluents of higher kinds than these, the Trajectories above-mentioned, which are described by a centripetal Force, that is, as some Power of the Distance from a given Centre, when the Velocity of the Projection is that which would be acquired by an infinite Descent, or by such a centrifugal Force, and the Velocity is such as would be acquired by flying from the Centre, are employed for representing them. A simple Construction of these Trajectories had been given above, by drawing Rays from the Centre to a Right Line given in Position, increasing or diminishing the Logarithms of those Rays always in a given

Ratio, and increasing or diminishing the Angles contained by them and the Perpendicular in the same *Ratio*. From any Figure of this kind a Series of Figures is derived by determining the Intersections of the Tangents of the Figure with the Perpendiculars from the Centre. Every Series of this kind gives Two distinct sort of Fluents; and any one Fluent being given, all the other Fluents taken alternately from it in the Series depend upon it, or are measured by it; but it does not appear, that the Fluents of one sort can be compared with those of the other sort, or with those of any different Series of this kind.

The inverse Method is prosecuted farther in the 4th Chapter, by various Theorems concerning the Area when the Ordinate is expressed by a Fluent, or when the Ordinate and Base are both expressed by Fluents. The First is the XIth Proposition of Sir *Isaac Newton's* Treatise of Quadratures. In Art. 819, 820, &c. the Author supposes the Ordinate and Base to be both expressed by Fluents, and shews, in many Cases, that the Area may be assigned by the Product of Two simple Fluents, as of Two circular Arcs, or of a circular Arc and a Logarithm. This Subject deserves to be prosecuted, because the Resolution of Problems is rendered more accurate and simple, by reducing Fluents to the Products of Fluents already known, than by having immediately recourse to infinite Series. One of the Examples in Art. 822. may be easily applied for demonstrating, that the Sum of the Fractions which have Unit for their common Numerator, and the Squares of the Numbers 1, 2, 3, 4, 5, 6, &c. in their natural Order; for

for their successive Denominators, is One-sixth Part of the Number, which expresses the *Ratio* of the Square of the Periphery of a Circle to the Square of its Diameter; which is deduced by Mr. *Euler*, *Comment. Petropol.* Tom. 7. in a different manner; and other Theorems of this kind may be demonstrated from the same or like Principles.

The Series that is deduced by the usual Methods for computing the Area or Fluent, converge in some Cases at so slow a Rate, as to be of little or no Use without some farther Artifice. For Example: The Sum of the first Thousand Terms of Lord *Brounker's* Series for the Logarithm of 2, is deficient in the fifth Decimal. In order therefore to render the Account of the inverse Method more complete, the Author shews how this may be remedied, in many Cases, by Theorems derived from the Method of Fluxions itself, which likewise serve for approximating readily to the Values of Progressions, and for resolving Problems that are commonly referred to other Methods. Those Theorems had been described in the First Book, Art. 352, &c. but the Demonstration and Examples were referred to this Place, as requiring a good deal of Computation. The Base being supposed equal to Unit, and its Fluxion also equal to Unit, let half the Sum of the extreme Ordinates be represented by a , the Difference of the first Fluxions of these Ordinates by b , the Difference of their Third, Fifth, Seventh and higher alternate Fluxions by c, d, e , &c. then the Area shall be equal to $a - \frac{b}{12} + \frac{c}{720} - \frac{d}{30240} + \frac{e}{1209600} - \dots$, &c. which is the first Theorem for finding the Area. The rest remaining,

let

let a now represent the middle Ordinate, and the Area shall be equal $a + \frac{b}{24} - \frac{7c}{5760} + \frac{31d}{967680} - \frac{127e}{154828800} + \&c.$

And this is the Theorem which the Author makes most Use of. When the several intermediate Ordinates represent the Terms of a Progression, the Area is computed from their Sum, or conversely their Sum is derived from the Area, by Theorems that easily flow from these.

These general Theorems are afterwards applied for finding the Sums of the Powers of any Terms in Arithmetical Progression, whether the Exponents of the Powers be Positive or Negative, and for finding the Sums of their Logarithm, and thereby determining the *Ratio* of the *Uncia* of the middle Term of a Binomial of a very high Power to the Sum of all the *Unciæ*. This last Problem was celebrated amongst Mathematicians some Years ago, and by endeavouring to resolve it by the Method of Fluxions the Author found those Theorems, which give the same Conclusions that are derived from other Methods. They are likewise applied for computing *Areas* nearly from a few equidistant Ordinates, and for interpolating the intermediate Terms of a Series, when the Nature of the Figure can be determined, whose Ordinates are as the Differences of the Terms.

In the last Chapter, the general Rules, derived from the Method of Fluxions for the Resolution of Problems, are described and illustrated by Examples. After the common Theorems concerning Tangents, the Rules for determining the greatest and least Ordinates, with the Points of contrary Flexure, and the Precautions that are necessary to render them accurate
and

and general, (which were described above) are again demonstrated. Next follow the Algebraic Rules for finding the Centre of Curvature, and determining the Causitics by Reflexion and Refraction, and the centripetal Forces. The Construction of the Trajectory is given, which is described by a Force that is inversely as the Fifth Power of the Distance from the Centre, because this Construction requires Hyperbolic and Elliptic Arcs, and because a remarkable Circumstance takes place in this Case, (and indeed in an Infinity of other Cases) which could not obtain in those that have been already constructed by others, *viz.* That a Body may continually descend in a spiral Line towards the Centre, and yet never approach so near to it as to descend to a Circle of a certain *Radius*; and a Body may recede for ever from the Centre, and yet never arise to a certain finite Altitude. The Construction of the Cases wherein this obtains is performed by Logarithms or Hyperbolic Areas, the Angles described about the Centre being always proportional to the Hyperbolic Sectors; while the Distances from the Centre are directly or inversely as the Tangents of the Hyperbola at its Vertex. The Circle is an Asymptote to the Spiral; and this can never be, unless the Velocities requisite to carry Bodies in Circles increase while the Distances decrease, (or decrease while the Distances increase) in a higher Proportion than the Velocity in the Trajectory; that is, unless the Force be inversely as a higher Power of the Distance than the Cube. Next follow Theorems for computing the Time of Descent in any Arc of a Curve, for finding the Resistance and Density of the Medium when the Trajectory and centripetal Force

Force are given, and for defining the *Catenaria* and Line of swiftest Descent in any Hypothesis of Gravity.

Then the usual Rules are derived from the inverse Method for computing the Area, the Solid generated by it, the Arc of the Curve, and the Surface described by it revolving about a given Axis. The meridional Parts in a Sphere, and any Spheroid, are determined with the same Accuracy, and almost equal Facility. The Attraction of a Spheroid at the Equator, as well as at the Poles, is determined in a more general manner than in the First Book, or in a Piece of the Author's published at *Paris* in 1740. which obtained a Part of the Prize proposed by the *Royal Academy of Sciences* for that Year. Several Mechanical Problems are resolved, concerning the Proportion the Power ought to bear to the Weight, that the Engine may produce the greatest Effect in a given Time; and concerning the most advantageous Position of a Plane which moves parallel to itself, that a Stream of Air or Water may impel it with the greatest Force, having regard to the Velocity which the Plane may have already acquired. On this Occasion, it is shewn, that the Wind ought to strike the Sails of a Wind-mill in a greater Angle than that of $54^{\circ} 44'$, against what has been deduced from the same Principles by a learned Author. The same Theory is applied to the Motion of Ships, abstracting from the Lee-way, but having regard to the Velocity of the Ship; and amongst other Conclusions it appears, that the Velocity of a Vessel of one Sail may be greater with a Side-wind, than when she sails directly before the Wind; which, perhaps, may be the Case of those seen by Captain
Dampier.

Dampier in the *Ladrone Islands*, that sailed at the Rate of 12 Miles in half an Hour with a Side-wind.


The Remainder of this Chapter is employed in reducing Equations from second to first Fluxions; constructing the elastic Curve by the Rectification of the equilateral Hyperbola; determining the Vibrations of Musical Chords; resolving Problems concerning the *Maxima* and *Minima*, that are proposed with Limitations, relating to the Perimeter of the Figure, its Area, the Solid generated by this Area, &c. with Examples of this kind concerning the Solid of least Resistance; and concludes with an Instance of the Theorems by which the Value of the Ordinate may be determined from the Value of the Area, by common Algebra, and by observing, that it is not absolute, but relative Space and Motion, that is supposed in the Method of Fluxions.

IV. *Observations on the Mouth of the Eels in Vinegar, and also a strange aquatic Animal, sent in a Letter from the Rev^d Mr. Henry Miles to Mr. Baker, F. R. S. and by him communicated to the ROYAL SOCIETY: With a Drawing and Description of the said Animal, as viewed in the Microscope, by Mr. Baker.*

S I R,

* * * *

Read March 10.
and 17. 1742-3.

I Have now to tell you an Observation I made on one of the *Anguillæ* in *Vinegar* (of which, by the way, I have a prodigious Increase, though I lost all by Accident about a Month since, to about a single Drop or two). The Observation was made with the *Camera Obscura* Microscope: First, in a very small Tube, not a Capillary one, though approaching near it, I put a small Quantity of *Vinegar* with several *Anguillæ*: At my first Sight of the Image on the Screen, I observed one to have a Motion as if it had been wounded, about the Middle of the Back, if I may so express it; it neither rose nor sunk in the Liquor, but lay in this Form  wriggling itself: I thought it gave Signs of Pain, and would soon expire, which it accordingly did in a Minute's time; but it coiled itself up, and stuck to the Side of the Tube very close, before I was aware: I put out the Liquor, after waiting to see whether it would revive, in vain, and viewed it

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several times in the common Light, which way I had the most distinct Appearance; and must acknowledge the Exactness with which it had coiled itself, gave me no small Pleasure to behold: It would be impossible for me to give you a Description of it, having no Skill in Drawing; but what I chiefly intended to tell you I will endeavour to describe as well as I can: The biggest End, which I call the Head, was stretched out from the rest of the Body, a little Way, as in the Figure, which gave me an Opportunity I had wished for, of examining what Mouth it had. On my first View of it in common Light, I saw what I incline to think may be called the Mouth: Repeated Trials in different Lights and Positions, and with different Magnifiers, confirmed my Suspicion; for I saw no other Appearance of it, than what I ought to expect on such Alterations of the Glasses, &c. I would only add, that after the strictest and most exact Observation I could make, I could discern it to be nothing more than a transparent Tube. Where the Instruments of Nutrition, and the Springs of Life, are, I doubt we shall not soon discover. I once indeed thought, viewing it in the *Camera*, &c. I saw a Blood-vessel, but I believe it was no more than refracted Light, or prismatic Colour.



a. The Mouth, which seemed to be as wide open as it possibly could be.

I am sensible my Figure is too small to give you a just Idea of the Shape of the Mouth, but it had the

Appearance which a Tube, or rather a Cone, would make cut sloopwise.

I have further troubled you with an odd aquatic Animal, some Specimens of which you will find inclosed, found in standing Water: I kept some of them in their own Element in the House, but they all died in a Day and half's time. They seem to be nothing but Skin, and seem no thicker when alive: They have the Power (as most aquatic Insects have) of sinking themselves to the Bottom on the Approach of a Stick, &c. and fall like a Piece of rotten Wood, or Leaf—When taken out of the Water, if laid on a Paper, &c. they will spring away like a *Grasshopper*. I do not at present remember ever to have seen them before, and know not what to make of them, unless they are the *Tipula*, or *Water-Spider*, not yet arrived at its mature State. I am,

Your most obliged

and very humble Servant,

Tooting, March
9. 1742-3.

H. Miles.

P. S. The Animals I send you were caught Yesterday, and kept in Water in a Glass; and when I had finished my Letter, I went to pack them up in Paper, but found none of them left, as I thought, at first; but on a nearer View I found they were all collected together in a Knot, which I took for some Filth in the Water, till I more carefully viewed them, and found them hanging together by the *Tails*.

Explanation of the FIGURE by Mr. Baker, see
TAB. IV. Fig. 3.

- A. Represents an undescribed Kind of aquatic Animal, (lately observed by the Rev^d Mr. Miles of Tooting) in the same Size and Form as it appears to the naked Eye. Several of them were found in the Water of a Ditch; whence being taken, and laid on a Paper, they had a leaping Motion.
- B. The same Animal, as examined by the Microscope, which shews it to be a triangular, oblong, opaque Body, somewhat like the Shape of a Prism, but tapering from End to End. The Three Horns (whereof those on the Sides are a Third Part longer than the Middle one) appear armed with extremely sharp Thorns or Prickles: The same Sort of Prickles are placed likewise along the Sides of the whole Body, pointing downwards, from Head to Tail.
- C. Shews the Form of a Body inclosed in the former, and taken out upon Dissection. This seems to be an Animal in its Aurelia State; and if so, what has been before described is only its Husk or Case, which will be quitted when it comes to change.

Quere, What Animal is this in its perfect State?

V. *Part of a Letter from Mr. Macky, Professor of History, to Mr. Mac Laurin, Professor of Mathematics in the University of Edinburgh, and by him communicated to the President of the ROYAL SOCIETY; being an Extract from the Books of the Town-Council of Edinburgh, relating to a Disease there, supposed to be Venereal, in the Year 1497.*

Read March
17. 1742-3.

IF the Venereal Disease was never known in *Europe* till the Siege of *Naples* 1495, it must have made a very quick Progress through *Europe* in a short time; for in 1497, I find it raging in *Edinburgh*, and our King and his Council terribly alarmed at this contagious Distemper, as appears from a Proclamation of King *James* the IVth, in the Records of the Town-Council of *Edinburgh*. The Minute of Council is dated the 22d of *September*. I have taken a Copy of it for your Amusement, and, if you please, you may communicate it to the SOCIETY. I have pretty nearly observed the old Spelling, except in Numbers.

“ 22. *Sept.* 1497.

“ IT is our Soverane Lords Will and the Command
“ of the Lordis of his Counsaile send to the Provost
“ and Baillies within this bur^t that this Proclamation
“ followand be put till execution for the eschewing
“ of the greit appearand danger of the Infection of
“ his Leiges fra this contagious sickness callit the
“ *Grandgor*

“ *Grandgor* and the greit uther Skayth that may
 “ occur to his Leiges and Inhabitans within this bur^t;
 “ that is to say, we charge straitly and commands
 “ be the Auhority above writtin, that all manner of
 “ personis being within the freedom of this bur^t
 “ quilks are infectit or hes been infectit uncurit with
 “ this said contagious plague callit the *Grandgor*, de-
 “ voyd, red and pafs furt of this Town and compcir
 “ apou the sandis of *Leith* at ten hours before none
 “ and thair fall thai have and fynd Botis reddie in the
 “ havin ordanit to them be the Officeris of this bur^t
 “ reddely furneist with victuals to have thame to the
 “ *Inche**, and thair to remane quhill God proviyd for
 “ thair Health: And that all uther personis the quilks
 “ taks upon thame to hale the said contagious infir-
 “ mitie and taks the cure thairof that they devoyd
 “ and pafs with thame sua that nane of thair personis
 “ quhilks taks sic cure upon thame use the samyn
 “ cure within this bur^t in pns nor peirt any manner
 “ of way. And wha sa beis foundin infectit and not
 “ passand to the *Inche* as said is be *Mononday* at the
 “ Sone ganging to, and in lykways the said personis
 “ that takis the sd Cure of sanitie upon thame gif
 “ they will use the samyn thai and ilk ane of thame
 “ falle be brynt on the cheik with the marking Irne
 “ that thai may be kennit in tym to cum and thair-
 “ after gif any of tham remanis that thai fall be
 “ banist but favors.”

* An Island in the *Firth* of *Edinburgh* over-against *Leith*.

VI. *Some Account of the Insect called the Fresh-water POLYPUS, before-mentioned in these Transactions**, as the same was delivered at a Meeting of the ROYAL SOCIETY, by the President, on Thursday, March 24. 1742-3.

GENTLEMEN,

THOUGH I can no-ways expect to add any thing of Consequence, to the very curious Account already † given you by Monsieur *Trembley*, of his Observations on the little Insects, I had the Pleasure of producing before you at your last Meeting; and though I am very sensible the Truth and Exactness of those Observations no ways want the Addition of any new Attestation, after those very full ones already given, by those worthy Members of this SOCIETY, both at the *Hague* and at *Paris*; the Honourable Mr. *Bentinck*, in his Letter to me, which I communicated to you, and Monsieur *Reaumur*, in the Preface to the Sixth Volume of his admirable History of Insects; and though both those Gentlemen have had the Advantage of seeing and making many more Experiments, than I can possibly have done in so short a time: yet, as I apprehend it is expected from me, that I should give some Account, how imperfect soever, of what I have already seen and taken notice of; and as I in some sort also owe my own Testi-

* N^o 466. p. 218.

† *Philosophical Transactions*, N^o 467.

mony as a Debt to the Truth, I shall without further Difficulty proceed to give it: And this I the more readily do, as Monsieur *Trembley* has favoured me with the Present of these small Bodies, and has taken care to have them conveyed over to me with the greatest Care, on purpose that I should examine them with Strictness, that I might truly report what I did really see; and that I might have it in my Power to shew them to others also, who should be willing to give any Attention to them, and convince themselves with their own Eyes of the very remarkable *Phænomena* they afford.

I received the Insects in Question, on *Thursday* the 10th of this instant *March*, in the Afternoon; the Water in which they were contained was grown foul at Sea, so that I immediately poured some of it off, and supplied it with fresh: After which going out presently to attend the Meeting of this SOCIETY, I could give them no further Attention till the next Morning; since which time I have missed as few Opportunities as possible, either of observing them myself, or of shewing them to such other of my Friends, as have done me the Honour of calling at my House.

I have found, that most of those I have particularly viewed, and that seem pretty well grown, have Ten Horns or Arms; but I have seen a few with Eleven, some others with no more than Nine, and one I have taken notice of, that had Fourteen: The lesser ones have frequently but Six of these Arms, and those have the fewest I have yet observed.

The Structure of the Arms, when looked at with the Microscope, is very curious: Each seems to consist

list of several Rows of Knots or small *Papillæ*, joined together by a transparent Membranous Substance, and which is endued with a most exquisite Power of Extension and Contraction; so as thereby to bring any of those Knots nearer together, or set them further asunder, and that in every possible Direction; whereby the Animal is able to bend any of these Arms in any Part, and all sorts of ways: Besides which, these Arms are also capable in the Whole of so great an Extension and Contraction, that I have frequently seen some of those of the same Creature extended, at one Moment, to more than Ten times the Length they were of at another.

The Body of the Insect is not much less capable of lengthening and shortening itself than the Arms. When most contracted, it looks like a little Ball, from one Part of which rises a small Knob, not unlike what is commonly seen at the Head of a Lemon: This is the Tail, and upon this the Insect in this Case generally rests: Opposite to this is the Mouth, round which the Arms appear regularly extended, and resemble a little Star, as usually represented, all whose Points seem to proceed from the same Centre. But, when extended, the same *Polyptus*, which, in the Position just described, scarce appeared One-tenth of an Inch in Diameter, has drawn itself out to full Three-quarters of an Inch in Length; in which State the Mouth does, for the most Part, project like a small and sharp Snout in the midst of the Arms.

Together with the Insects, Monsieur *Trembley* sent me over some very small Water-worms, which he informed me they readily preyed upon; and these
Worms

Worms I have several times had the Pleasure of seeing them seize with great Dexterity and Eagerness; soon after which they have sucked them in, and swallowed them completely down, though apparently several times larger than themselves.

Mr. *Trembley* has, in the Fifth Page of his Relation just published in the last *Philosophical Transactions*, given a very exact and curious Description of what concerns this last Particular, of their taking and devouring their Prey: To which I shall only add, that it appears to me, that the little *Papillæ* above described on the Surface of the Arms assist them like so many Hooks or Tenters to hold their Worms barely by touching them; for I have more than once seen a *Polypus* draw a Worm to him, and nimbly turn it about with a single Arm, only laid over it, without folding or clasping it; which last Method, however, he makes use of also, when the Worm comes to struggle and strive hard to be disengaged.

Generally before the *Polypus* fixes on the Worm with his Mouth, the Mouth and his whole Fore-part begins to extend itself; and after fastening upon it, which is frequently near the Middle, the whole Body swells, the Worm commonly appears bloody, and the *Polypus* sucks down a great deal of the Blood and Juice, before he begins to swallow the Worm itself: During all this Time he continues to extend and stretch his Mouth, and that to such a Degree, that I have seen its Breadth, when in the Act of first bending in a Worm seized by the Middle, not less than the whole Length of the Animal when in a mean State of Extension.

In the Scituation juſt mentioned, the Mouth reſembles an open Cup; and there is a conſpicious Neck between that and the Belly, which then ſwells out like that of a *Morence* Flaſk; beyond which again appears the Tail, not ſtretched in proportion to the reſt, but whoſe Cavity, when the Inſect is made tranſparent, appears to the Microſcope as a Gut running from the Stomach, but which has ſeemed to be a *Cæcum**, and not open at the lower Extremity; nor have I ever yet ſeen any thing like an Evacuation that Way.

As the *Polypus* gets the Worm to double, and draws it further in, the Neck, juſt mentioned, ſwells, and the Mouth ſomewhat contracts again, ſo that the whole Body puts on the Appearance of a ſort of Purſe or Pouch; but the Tail never intirely diſappears, though it ſhortens remarkably, on the Swelling of the Gut with the Juice drawn from the Worm: But into this Gut I have never ſeen any of the ſolid Part of the Worm to penetrate, though I have often ſeen its whole Body lie coiled up in what I have looked upon as the Stomach of the Inſect.

He lies for the moſt part pretty ſtill during the latter Part of his Meal, like a Creature gorged with too much Food, drawing in the Worm ſlowly at laſt: But after it is all got in, he again contracts his Mouth, and ſtretches his Neck-part in Length, as it were, to compoſe the Poſture of the Worm in his Stomach,

* This has ſince appeared to be a Miſtake; the Gut is alſo open at the lower End, and though the larger *Fæces* are all thrown up again by the Mouth, I have ſince ſeen a thinner Slime evacuated that Way.

where it continues to lie till digested; it soon loses its Distinctness, and its Shape becomes in a little time undiscernible; the *Fæces*, however, are not thrown off till several Hours after, when they come away by the Mouth in the Form of small Pellets of Cobweb, which I have not yet actually seen thrown out, though I have several times seen them before they were thoroughly disengaged from that Part.

A *Polypus*, when in a middling State of Contraction, shews to the Microscope, much like a Slug or long Snail: His Sides are wrinkled, and he then appears as if made up of Rings, like a Grub or Earthworm; but these Rings all disappear when the Insect is more extended, his whole Skin then looking as beset with little *Papillæ*, like those of his Horns or Arms, except that they are smaller.

When he hangs fixed to any thing by the Tail, his most usual Posture, he will turn his Body in all Ways, coiling and writhing it about, so as sometimes to stroke, as it were, his Tail with his Arms, and rub it with his Mouth, as if to remove some Uneasiness, possibly given him by lesser Water-insects, which I have often observed like Lice crawling upon his Body. A progressive Motion I have also sometimes seen, when he helps himself alternately with his Arms and Tail, but this sort of Motion is less frequent than his others.

I fear I have dwelt too long on these little Particulars, which I was however willing to take notice of, as they may serve to shew the *Polypus* is really and truly a living Creature, and, like other small Insects, provided with proper Parts and Organs for the catching, eating, and digesting, of his Food: For though the
Pro-

Production of the young ones from the Sides of the Parent has a near Resemblance to the shooting of the Branches from the Trunks of Vegetables, and though some other of his Properties are so very singular and surprising; yet all those above-mentioned and described, are without all Doubt Animal Operations.

This Sprouting of the Young Ones from the Sides of the others, is already so fully described by Monsieur *Trembley* himself, that I have very little to add to that Description, farther than to observe, that the young ones I have seen shoot out, had no Arms till they had acquired some Length: Those I have had the Beginning of before me, have not shewed them till about the Fifth Day from the first Appearance, but this might probably vary in a warmer Season.

As soon as the little ones have Arms, they will themselves take and eat Worms while fixed; and it appears, that during that Time, the Gut of the little one opens into and joins the Gut of the Parent: This I hope indeed to confirm by some further Experiments; but it has constantly appeared to me, that upon the little ones eating, the Stomach and Gut of the Parent has become extended also, and *vice versa*.

I have had one *Polypus*, that had Three young ones dependent from him at the same time, and one of these young ones has begun to put out a young one itself, so that they formed a Cluster of Five of these Insects hanging together: But one of the young ones separated itself, and dropt off Yesterday Morning; and this Morning I perceive another little one just breaking out.

I shall now proceed to what I have tried with regard to the dividing of these Animals, and the Reproduction of their Parts.

On *Sunday* the 13th of this Instant *March*, I chose a long slender *Polypus*, that appeared lively, but that had not been fed since I received it; and putting it with a Drop of Water in the Palm of my Left Hand, I watched the Time of its extending itself, and then with my Scissars cut it asunder into Two Parts, near the Middle; both which Parts I put separately into Two Phials of *New-River-water*. This was done about Two o'Clock in the Afternoon.

On *Monday* the 14th, I observed the Arms on the Head-part to play; the Tail-part lay along on the Bottom of the Phial, but looked plump, and from time to time alternately extended and contracted itself: The Wounds of both Parts appeared contracted and drawn together.

On *Tuesday* the 15th, the Head-part seeming active and busy with its Arms, I gave it, about Nine in the Morning, a small Piece of a Worm; it very readily seized it, and presently after eat it: I viewed this Part carefully with a Magnifying-glass, and found the Wound no-ways affected by the Extension of the Stomach. The Wound of the Tail-piece appeared well rounded off.

On *Wednesday* the 16th, the Head-piece seemed very well. The Tail-piece stirred very remarkably, and its wounded End shewed in Shape like that of a little turned Nine-Pin.

On *Thursday* the 17th, I saw the Head-piece raised up and resting on its posterior End, as before it was hurt. The Tail-piece discovered a very remarkable

able Rounding off at the wounded End, which looked also somewhat extended, and more pellucid than the rest. It both extended and contracted itself very sensibly, moved more frequently than it had yet done, and I observed a small Protrusion towards the Middle of its Length, which I fancied like the Beginning of a young one just putting out from that Part.

On *Friday* the 18th, about Seven in the Morning, I perceived little Horns or Arms putting out from the wounded End of the Tail-part: They were yet very short, but shewed themselves distinctly all round, and I could see them play very clearly. The Protrusion on the Side was enlarged, so as now to be known evidently for a new *Polypus*. The Head-part seemed very well; and in the Afternoon the Arms of the other Part were sensibly lengthened.

On *Saturday* the 19th, I found the new Arms yet longer: I now gave a Piece of a Worm to this Part. It readily hooked it, and eat it. The little one was very conspicuous, but that it yet wanted Arms.

On *Sunday* the 20th, every thing was improved, and small Arms began to discover themselves, on the little one sprung from the Side of the Tail-piece.

On *Monday* the 21st, both Pieces appeared perfectly well, they had all the Appearance of perfect intire Insects, the same as before they were cut, and continue as fair and as good as any I have. The little one is not yet dropt off.

I have been very particular in this Account, from the Minutes I took down every Day; and I shall further observe, that I cut Three more transversely in the same Manner, on the same Day, *Sunday* the 13th Instant, and that I had so cut one on the Day before:

before: They all went on nearly in the same Manner, and all shewed the new Arms on their Tail-parts on the same *Friday* the 18th; but I must also take Notice, that *Thursday* last the 17th was a fine warm Day, to which I impute it, that the Insects cut on *Sunday* were just as forward as that cut on the Day before. One other of the Tail-pieces of these also put forth a young one, during the time that it lay without a Head. All these Four last-mentioned had eat about 36 Hours before they underwent the Operation.

On *Tuesday* the 15th Instant, I took a *Polypus* that had eat a Worm on the *Saturday*, and, placing it as before in the Hollow of my Left Hand, I attempted, when it was most contracted, to divide it longitudinally; but my Scissars not being very good, I miss'd my Stroke, was forced to give a Second, and even then divided it very unequally; the Head was however split, and of Ten Horns that it had, Six came off with the lesser Piece that was only a Slip of the Body, and the Four others remained with the rest, which was at least Seven-eighths of that Body. I had very small Expectation from this Experiment, I nevertheless put both the Pieces with some Water into a Phial; and both this Day in the Afternoon and the next, I saw both Parts playing their Arms.

On *Thursday* the 17th, in the Forenoon, perceiving both these Parts to move their Horns pretty briskly, I gave to each a Piece of a Worm: Each readily seized it, eat it, and kept it as usual; and the same Day in the Afternoon, I took Notice, that a little one was putting out from the Side of the larger Piece.

On *Saturday* the 19th, I saw both the Pieces resting on their posterior Ends, and stretching out their Bodies in the usual way.

On *Monday* the 21st, both Parts seemed well, each was like an intire *Polypus*, except that one of them was, and is still, very small. I discovered some little Arms putting out in the room of those each Part had lost: There appeared also little Arms coming out all round the Head of the little one fixed to the Tail of the larger Piece.

On *Tuesday* last the 22d, I viewed both these Pieces with the Microscope, and each seemed perfectly formed like a whole *Polypus*. The larger Piece had Four new Arms, and the lesser Two, like their others, but shorter, as yet; and they are now in all other respects, as complete as any others I have.

This same Experiment I again attempted on another *Polypus*, on *Saturday* last; but I again made the Parts unequal; they are however both alive, and promise very fairly.

I the same Day cut a fine long *Polypus* into Three Pieces, transversely, at Five in the Afternoon. I left the Middle-piece the longest of the Three.

On *Monday* the 21st, the Head-piece seemed well formed again, excepting that it was yet very short; the other Pieces looked plump and well. On *Tuesday* the Head-piece eat and kept Part of a Worm: It seized it very vigorously with its Arms, mastered it, and swallowed it cagerly. The Middle-piece moved pretty much, and the last looked fresh and well. This *Thursday* Morning, the Arms begin to shew themselves on the anterior Extremity of the Middle-piece.

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What I have related is a faithful Account of all the Experiments I have yet had Time to make on the *Polypus*. Those above-mentioned are all that I have cut; by which it appears, that I have yet had no unsuccessful Operation; and I am not conscious, that I have yet lost one *Polypus* by any Accident, though the Weather has been very severe, excepting one Day, ever since they came over; and this has probably made things go on slower, and the Reproductions require longer Time than it is reasonable to think they would have done, in a more favourable and milder Season.

All I can say for my Experiments, is, that I have made them with as much Care and Circumspection as I was Master of; and that, in this Relation of them, I have in the strictest manner adhered to the exact Truth, in the most minute Particulars: I must also add, that the Insects which have been cut, or upon which I have made any sort of Experiment, have never been out of my own private keeping, and have never been so much as seen by any Person whatsoever but in my own Presence.

I should now, GENTLEMEN, make an End, and ask your Excuses for troubling you with so long an Account, were I not in some sort obliged, before I conclude, to return my Thanks to a very worthy Member of our SOCIETY, Dr. *Parsons*, whose extreme Curiosity, and earnest Desire to promote the Knowledge of Nature, has prompted him to favour me with his Company and Assistance in several of my Experiments; besides which he has also furnished me with some very exact and curious Drawings of these minute Bodies, as they have appeared to the

Microscope in various Altitudes, and under different Circumstances; an Assistance I could hardly have received from any other Hand, as this Gentleman joins the greatest Accuracy in observing the true Structure of the Works of Nature, with a peculiar Happiness in representing them. And these Drawings I have brought with me, for the Entertainment of the GENTLEMEN present; and to whom I am persuaded they will not be less pleasing and satisfactory, than they have been to myself.

After which Mr. *Cuff*, who has at my Request brought hither an excellent Microscope, will be so kind as to endeavour at shewing the Insect itself to such GENTLEMEN, as not having yet seen it, may now be willing to take a View of it in that Manner. But as it may be found difficult for the whole Company to satisfy their Curiosity here sufficiently at the same time: I must again repeat what I had the Honour of saying to you the last *Thursday*, that so long as I shall have any of these little Animals in my Possession, I shall always be ready and desirous to shew them, at my own House, and give the best Satisfaction I am able, to any GENTLEMEN disposed to take further Notice of them.

References to the Figures above-mentioned.

TAB. V.

Fig. 1. *Represents a Polypus as seen in the Microscope, when in a State of Extension, the Arms spread as when feeling for their Prey, and the Mouth sharp and prominent.*

Fig. 2. and 3. *Represents the same Insect in its most contracted State.*

Fig. 4. and 5. *Shews the Inſect when in a middle State of Contraſtion; the Body is then wrinkled, ſo as to appear ſomewhat like a Grub or Earth-worm.*

Fig. 6. *Is a Polypus with a young one growing from its Side, and another from that again: This is not ſo much extended as that in Fig. 1. and is to be ſuppoſed to have taken lately ſome Food, whereby the Cavity of the Inſide is made more conſpicious, and the Communication of the Guts of the young ones with thoſe of the Parents becomes ſenſible.*

Fig. 7. *Shews the Appearance of a Polypus, that has already ſwallowed the beſt Part of a Worm endways. He is graſping the remaining Part to draw that in alſo.*

Fig. 8. *Represents a Polypus, whoſe Mouth is greatly extended: He has juſt taken in the middle Part of a Worm; the Opening of the Mouth is there remarkable, the Arms ſeem ſomewhat contracted from the Effort in ſtretching the Mouth ſo wide; the Neck alſo may be there obſerved between the Mouth and the Stomach, but which will ſoon diſappear as the Worm is ſucked further in.*

Fig. 9. *Is another Polypus, nearly in the ſame State as the laſt; but the Worm is omitted in the Figure, to ſhew the Form of the Mouth more diſtinctly.*

Fig. 10. *Shews the ſame Polypus when the Worm is drawn quite double into his Stomach; here the Neck intirely diſappears, and the Whole is like an open Bag or Purſe.*

Fig. 11. *The ſame Polypus, after he has intirely ſwallowed his Worm; the Mouth is now again cloſed and*

and contracted, and the Worm may be discovered through the Skin, as it lies coiled in his Stomach. In these five last Figures it may be noted, that, however extended and swelled the Stomach of the Insect appears, the posterior Part is not stretch'd in Proportion, but discovers itself every-where as a small Tail, in which is contained a Gut, with which the Stomach communicates.

Fig. 12. *Shews one of the Horns or Arms of a Polypus very much magnified, for the giving some imperfect Idea of the Knots or Papillæ in the transparent membranous Substance, of which it is composed.*

TAB. VI.

Represents a Polypus that had several Young growing from him at once, some of which had also others springing out from them again. This was the same Polypus mentioned in the foregoing Paper to have had Three young ones dependent from him at the same time, but which, becoming still more fruitful, was drawn a few Days after as he appears in this Figure, and when, besides those here represented, Eight other young ones had at several times separated themselves from him, since I received the Insects.

VII. *An Account of a Book intituled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Investigation of the resisting Power of the Air to swift and slow Motions; by B. R. F. R. S. as far as the same relates to the Force of Gunpowder.*

*Read April 14.
and 21. 1743.*

THIS Treatise contains Two Chapters. The First treats of the Force of Gunpowder, and the Velocities communicated to Bullets by its Explosion: The Second considers the Resistance of the Air to Bullets and Shells moving with great Velocities; and endeavours to evince, that this Resistance is much beyond what it is generally esteemed to be; and consequently that the Tract described by the Flight of these Projectiles, is very different from what is usually supposed by the modern Writers on this Subject.

The principal Points endeavoured to be established in the First Chapter are these, “ That the Force of
“ fired Gunpowder is no more than the Action of a
“ permanent elastic Fluid, which is produced by the
“ Explosion; that this Fluid observes the same Laws
“ with common Air in their Exertion of its Pressure
“ or Elasticity;” and consequently, “ That the Ve-
“ locities communicated to Bullets by the Explosion
“ may be easily computed from the common Rules,
“ which are established for the Determination of the
“ Air’s Elasticity.”

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The Two first Propositions contain the Proofs that a permanent elastic Fluid is constantly generated in the Explosion of Gunpowder; this is evinced by well-known Experiments daily repeated, and acquiesced in by all who have frequented the usual Courses of Experimental Philosophy, of which these Experiments generally make a Part; so that the Author presumes he may consider this Point as incontrovertibly established, at least he has never yet met with any who have questioned it.

The Third Proposition is, That the Elasticity of this Fluid produced by the Firing of Gunpowder, is, *cæteris paribus*, directly as its Density; and the Experiment by which this was confirmed, was letting fall separately Two Quantities of Powder, the one double the other, on a red-hot Iron included in an exhausted Receiver; and it appeared by the Descent of the Mercury, that the Elasticity of the Fluid produced from the double Quantity of Powder, was nearly double the Elasticity of that produced from the single Quantity; that is, the Elasticity was nearly as the Density of the Fluid.

But it may perhaps be thought, that a single Experiment is too slender a Foundation on which to build so material a Principle, since all subsequent Reasonings on the Force of Powder in some measure depend on it. In Reply to this it may be said, that the Author recited this single Experiment on account of the great Quantity of Powder made use of in it, which was Three-sixteenths of an Ounce; but that he had really made many more equally conclusive, which he thought it unnecessary to mention. However, those who doubt of this Proposition, may
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satisfy themselves herein by some Experiments made by the late Mr. *Hauksbee* before this SOCIETY, though with a different View; where, by the firing of Twenty-six Quantities of Powder successively, the mercurial Gage was sunk from Twenty-nine Inches and an half, to Twelve Three-fourths; for by comparing these Experiments together, and making the necessary Allowances, it will be found, that the Elasticity was nearly proportional to the Density in all that Variety of Densities.

In this Proposition, the Analogy between the Fluid produced by the Explosion of Powder and common Air, is established thus far, that they exert equal Elasticities in like Circumstances; for this Variation of the Elasticity, in proportion to the Density, is a well-known Property of common Air. But other Authors, who, since the Time of Mr. *Boyle*, have examined the factitious elastic Fluids produced by Burning, Distillation, &c. have carried this Analogy much farther, and have supposed these Fluids to be real Air, endued with all the Properties of that we breathe; particularly the Reverend Dr. *Hales*, who has pursued this Examination with the greatest Exactness, in a Series of the best contrived Processes, constantly affixes the Denomination of Air to these factitious Fluids, he having found, that their Weight is the same with that of common Air, and that they dilate with Heat, and contract with Cold; and that they vary their Densities under different Degrees of Impression in the same Proportion with common Air; and from hence, and other Circumstances of Agreement between them, he supposes them to be of
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the same Nature with Air, and conceives them to be fitly designed by the same Name.

But so perfect a Congruity between these factitious Fluids and Air is not necessary for the Purposes, of this Treatise. The fundamental Positions of this First Chapter supposing no more, than that the Elasticity of the Fluid produced in the Explosion of Gunpowder is always, *cæteris paribus*, as its Density; and that the Force of fired Gunpowder is only the Action of that Fluid modified according to this Law. It has been already mentioned, on what Grounds the First of these Principles hath been asserted, as contained in the Third Proposition; and it remains to explain the Reasons urged for the Support of the last in the Eight succeeding Propositions.

The Law of the Action of this Fluid being determined, Two Methods offer themselves for investigating the absolute Force of Powder on the Bodies it impels before it. The first by examining the Quantity of this Fluid produced by a given Quantity of Powder, and thence finding its Elasticity at the Instant of the Explosion; the other by determining the actual Velocities communicated to Bullets by known Charges, acting through Barrels of different Dimensions. The First is the most easy and obvious, but the Second the most accurate Method; and therefore the Author has separately pursued each, and he has found, that their Concurrence has greatly exceeded his Expectation, and thereby both of them receive an additional Confirmation.

The Quantity of the elastic Fluid, produced by the Firing of a given Quantity of Powder, is determined
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by firing it in an exhausted Receiver, and observing how much the mercurial Gage subsides thereby, making a proper Allowance for the Increase of its Elasticity from the Heat of the included hot Iron. But then, as the Subsiding of the Mercury is not measured till the Flame of the Powder is extinguished, and the Fluid is reduced somewhat near the Temperature of the external Air, it is evident, that the Elasticity thus estimated is much short of what it really was in the Instant of Explosion; and therefore, to obtain that Elasticity, which is the Force sought, it is necessary to make some Estimate of the Increase of the Elasticity of the Fluid by the Fire and Flame of the Explosion. For this Purpose it is examined in the Fifth Proposition, how much the Elasticity of common Air is increased by a Degree of Heat equal to that of Iron beginning to grow white hot; and it is found, at a Medium, to be thereby augmented something more than Four times; whence, as the Fluid produced by any Quantity of Gunpowder takes up, when compressed by the Weight of the incumbent Atmosphere, a Space something less than 250 times the Bulk of the Powder; it follows, that if its Elasticity in the Instant of Explosion be supposed to be increased in the same Proportion with that of the Air last-mentioned, it becomes by this means about 1000 times greater than the Pressure of the Atmosphere; that is, conceiving it to be contained in that Space only which the Powder occupied before it was fired.

Those who have not been conversant in these Experiments, may possibly suppose, that the Elasticity of the Powder at the Instant of Explosion may be im-

mediately known by the First sudden Descent of the Mercury: But many Circumstances concur to render this Method impracticable; amongst the rest it must be remembered, that some Air is constantly left in the Receiver, which is heated by the Blast, and unites its Effects in the First Instant with the Action of the Powder: Besides, the First Descent may be varied, by varying the Tube, although all things else remain unchanged.

By the Method hitherto described, it is collected, that the Elasticity of the Fluid produced from fired Gunpowder, when contained in the Space which was taken up by the Powder before the Explosion, is about 1000 times greater than the Elasticity of common Air, or, which is the same thing, 1000 times greater than the Pressure of the Atmosphere.

But, besides the Determination of the Quantity of Fluid produced from a given Quantity of Powder, (the Method on which this Deduction is founded) there is another Method of discovering the same thing, which, though less obvious, is yet (as hath been already observed) more accurate: That is, by examining the actual Velocities communicated to Bullets by the Explosion of given Charges in given Cylinders; and this is the Subject of the 7th, 8th, and 9th Propositions.

And First, it is evident, that this Examination cannot take place, unless a Method of discovering the Velocities of Bullets be previously established. Now the only known Means of effecting this was, either by observing the Time of the Flight of Bullets through a given Space; or by finding their Ranges when they were projected at a given Angle, and thence computing

puting their Velocity on the Hypothesis of their parabolic Motion. The First of these Methods was often impracticable, and in all great Velocities extremely inaccurate, both on account of the Shortness of the Time of their Flight, and the Resistance of the Air. The Second is still more exceptionable, since, by reason of the Air's Resistance, the Velocities thus found may be less in any *Ratio* given, than the real Velocity sought. Now, to avoid these Difficulties, the Author has invented a Method of determining the Velocities of Bullets, which may be carried to any required Degree of Exactness, and is no-ways liable to the forementioned Exceptions; for, by this Invention, the Velocity of the Bullet is found in any Point of its Track, independent of the Velocity it had before it arrived at that Point, or of the Velocity it would have after it had passed it: So that not only the original Velocity, with which it issues from the Piece, is hence known, but also its Velocity, after it has passed to any given Distance; and therefore the Variations of its Velocity from the Resistance of the Air may be also ascertained with great Facility. The Machine for this Purpose is described in the 8th Proposition, and the Principle it is founded on is this simple Axiom of Mechanics; *That if a Body in Motion strikes on another at Rest, and they are not separated after the Stroke, but move on with one common Motion, then that common Motion is equal to the Motion with which the First Body moved before the Stroke*: Whence, if that common Motion, and the Masses of the Two Bodies are known, the Motion of the First Body before the Stroke is thence determined. On this Principle then it follows, that
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the Velocity of a Bullet may be diminished in any given *Ratio*, by its being made to impinge on a Body of a Weight properly proportioned to it; and hereby the most violent Motions, which would otherwise escape our Examination, are easily determined by these retarded Motions, which have a given Relation to them. Hence then, if a heavy Body greatly exceeding the Weight of the Bullet, whose Velocity is wanted, be suspended so that it may vibrate freely on an Axis in the manner of a Pendulum, and the Bullet impinges on it when it is at Rest, the Velocity of the Pendulum after the Stroke will be easily known by the Extent of its Vibration, and from thence, and the known Relation of the Weight of the Bullet and the Pendulum, and the Position of the Axis of Oscillation, the Velocity with which the Bullet is impinged will be determined, as is largely explained in the 8th Proposition. Where note, that there is a Paragraph by Mistake omitted in that Proposition, which should increase the Velocity there found in the duplicate Proportion of the Distances of the Points of Oscillation and Percussion from the Axis of Suspension; but this only affects that particular Number, for it was remembered in the Computations of the succeeding Experiments, the Numbers of which are truly stated.

It being explained how the Velocities of Bullets may be discovered by Experiment: The next Consideration is, from those Velocities to determine the Force which produced them.

And the Author thought, the best Method of effecting this was by computing what Velocities would arise from the Action of fired Powder, supposing its
Force

Force to be rightly assumed by the Process in the preceding Part; that is, supposing the Elasticity of the Fluid thence arising to be at first 1000 times greater than that of common Air; for then, by comparing the Result of these Computations with a great Number of different Experiments, it would appear whether that Force was rightly assigned; and if not, in what Degree it was to be corrected.

Preparatory to this Computation, the Author assumes in his 7th Proposition these Two Principles:

1st, That the Action of the Powder on the Bullet ceases as soon as the Bullet is got out of the Piece.

2^{dly}, That all the Powder of the Charge is fired, and converted into an elastic Fluid, before the Bullet is sensibly moved from its Place.

And in the annexed Scholium he has given the Arguments and Experiments which induced him to rely on these Postulates, all which is necessary at present to discuss more at large.

If the Force of Gunpowder was supposed capable of being determined with the same Accuracy and Rigour, which takes place in Subjects purely Geometrical, the First of these Postulates would be doubtless erroneous, since it cannot be questioned but the Flame acts in some Degree on the Bullet after it is out of the Piece.

But it is well known, that in Experimental Subjects no such Preciseness is attainable; for those versed in Experiments perpetually find, that either the unavoidable Irregularities of their Materials, or the Variation of some unobserved Circumstance, occasion very discernible Differences in the Event of similar Trials. Thus the Experiments made use of
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for confirming the Laws of the Collision of Bodies, have never been found absolutely to coincide either with the Theory, or with each other. The same is true of the Experiments on the Running and Spouting of Water and other Fluids, and of the Experiments made by Sir *Isaac Newton*, for the Confirmation of his Theory of Resistances; in which, though they often differ from each other, and from that Theory by One-twentieth, One-tenth, and even sometimes One-fifth Part, yet those small Inequalities have never been urged as invalidating his Conclusions, since, in Experiments of that Nature, it was rather to be wondered at, that the Difference between the different Trials was so small.

And if some minute Irregularities are the necessary Concomitants of all complicated Experiments, it may be well supposed, that the Action of so furious a Power as that of fired Gunpowder, which visibly agitates and disorders all Parts of the Apparatus made use of, cannot but be attended with sensible Variations; and it in Fact appears, that in the Table of Experiments inserted in the 9th Proposition, the Velocities of Bullets fired from the same Piece, charged with the same Powder, and all Circumstances as near as possible the same, do yet differ from each other by One-fiftieth, One-fortieth, and sometimes more than One-thirtieth, of the Whole; and yet the Author does not conceive, that these small Differences are any Exception to the Conclusiveness of his Principles; but he presumes, that had he pretended, without disclosing his Method, to have computed the Force of Powder, and the Velocities of Bullets, in different Circumstances, to a much less Degree of Accuracy
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than this, he should have been censured, as boasting of what would have been thought impracticable.

If then the Action of the Flame on the Bullet after it is out of the Piece, is so small as to produce no greater an Effect than what may be destroyed by the inevitable Variations of the Experiments, the neglecting it intirely, and supposing no such Force to take place, is both a convenient and a reasonable Procedure: For indeed, without the Assumption of Postulates of this kind, it were impossible to have proceeded one Step in Natural Philosophy, since no Mechanic Problem hath been ever solved, in which every real Inequality of the moving Force hath been considered.

Now what induced the Author to suppose, that this Postulate (though not rigorously true) might be safely assumed, was the Consideration of the spreading of the Flame by its own Elasticity, as soon as it escapes from the Mouth of the Piece: For by this means he conceived that the Part of it which impinged on the Bullet might be safely neglected, although the Impulse of the intire Flame was a very remarkable Force.

With regard to the Second Postulate, " That all
 " the Powder is fired before the Bullet is sensibly
 " moved from its Place;" it is incumbent on the Author to be still more explicit, as this SOCIETY did some time since appoint a Committee for examining this very Position, who, after making a great Number of Experiments, have determined, * *That all the Powder is not fired before the Bullet is sensibly*

* See these *Transactions*, N^o 465. p. 172, &c.

moved from its Place; and they have at the same time assigned the Quantities remaining unfired under different Circumstances.

These Determinations of the Committee are most true; but the Author must observe, that from the Experiments recited by them, and the Quantity of unfired Powder, which they collected, it may be concluded, that in a Barrel of a customary Length, charged with the usual Quantity of Powder, the Deficiency of Velocity occasioned by the Powder remaining unfired will be scarcely sensible; and in the shortest Barrel ever used by the Author, where the Space the Bullet was impelled through was not Five Inches, and where of course this Deficiency of Velocity ought to be the greatest, it cannot amount to One-thirtieth Part of the Whole; and consequently this Postulate, though not rigorously true, may yet be safely assumed, in the investigating the Effects of Powder. But before this is more particularly examined, it is necessary to explain the Opinions, which have formerly taken place on this Subject.

Those who have hitherto wrote on the Manner in which Powder takes Fire, have supposed it to be done by regular Degrees; the First Grains firing those contiguous, and they the next successively; and it has been generally thought, that a considerable Time was employed in these various Communications: For Mr. *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from some Experiments made at *Petersburgh*, that the greatest Part of the Charge escapes out of the Piece unfired, and that the small Part, which is fired, does not take Fire till it is near the Mouth. Many Theories too have been
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composed on the Time of the Progress of the Fire amongst the Grains, and the different Modifications which the Force of Powder did thence receive; and it has been generally conceived, that the proper Lengths of Pieces were determinable from this Principle; "That they should be long enough to give "Time for all the Powder to fire."

But the Author being satisfied, that no such regular and progressive Steps could be observed in the Explosion; and having found, that by loading with a greater Weight of Bullet, and thereby almost doubling the Time of the Continuance of the Powder in the Barrel, its Force received but an inconsiderable Augmentation; and finding too, that doubling or trebling the usual Charge, the Powder thus added always produced a correspondent Effect in the Velocity of the Bullet; and discovering likewise in a Piece near Four Feet in Length, charged with an usual Charge of Powder, that the Velocity communicated to the Bullet, during the First Three Inches of its Motion, was full half the Velocity which it acquired in its whole Passage through the Barrel, and that the Elasticity or Force of the Powder, in the First Three Inches of its Expansion, was, at a Medium, near Eight times greater than in the last Two Feet of the Barrel; he concluded from all these Circumstances, that the Time employed by the Powder in taking Fire was not necessary to be attended to in these Computations; but that the whole Mass might be supposed to be kindled, before the Bullet was sensibly moved from its Place.

And the Experiments reported by the Committee are the strongest Proofs, (as far as they extend) that

Powder is not fired in the progressive Manner usually supposed; for when the short Barrel was charged with 12 *dwt.* and with 6 *dwt.* respectively, the Quantity of Powder which was collected unfired from 12 *dwt.* did not exceed by 3 Grains, at a Medium, what was collected from 6 *dwt.* although the Bullet was a less Time in passing through the Barrel with 12 *dwt.* than with 6 *dwt.* it having a less Way to move; consequently the Quantity remaining unfired of the 6 *dwt.* did not continue unfired for want of Time, since, when the Piece was charged with 12 *dwt.* the additional 6 *dwt.* was consumed in a shorter Time.

And again, when the Barrel was so shortened, that the Bullet, being placed close to the Wad, lay with its outer Surface nearly level with the Mouth of the Piece, so that it had not more than half an Inch to move before the Flame would have Liberty to expand itself; yet, even in this short Transit of the Bullet, only 2 *dwt.* 1 $\frac{1}{2}$ *gr.* was collected unfired, at a Medium; which is about $\frac{1}{6}$ of the whole Charge, or, if properly reduced, not more than $\frac{1}{12}$ of the Charge: An obvious Confutation of the gradual Firing of the Powder in its Passage through the Barrel, and an easy Proof, how small an Error will be occasioned by supposing the whole Charge to fire instantaneously, since the Error in the Velocity of the Bullet, arising from a Deficiency of $\frac{1}{12}$ of the Charge, is $\frac{1}{24}$ of that Velocity only.

I say, that the $\frac{1}{6}$ of the Charge, which remained unfired, amounts to no more than $\frac{1}{12}$ when it is reduced at it ought. This Reduction is founded on
the

the other Experiments reported by the Committee, and on the Circumstances of those Trials on which the Author founded the present Postulate. The Author has supposed the Powder, on which he reasons in this Treatise, to be of the same sort with that made for the Service of the Government, a Parcel of which he was favoured with by Mr. *Walton*. But this he chiefly kept for a Standard, and generally used other Powders, which, on Examination, he found to be of equal Force. These Powders were of a very small and even Grain, and the Committee have found, that by sifting the Government Powder, and making use of the smaller Grains, the Quantity remaining unfired was less, at a Medium, in the *Ratio* of 5 to 3, than when it was used without sifting.

And again, it was found by extracting the Saltpetre from the Powder collected unfired, that there was less Saltpetre contained in it than in real Powder, and this nearly in the *Ratio* of 9 to 7: These Two Proportions compounded make the Proportion of 15 to 7, and in this Proportion must the Quantities of Powder collected unfired be reduced, in order to determine the Quantities of real Powder remaining unfired, in similar Experiments made by the Author.

And from hence it follows, that in the Experiments made with a Barrel $5\frac{1}{2}$ Inches in Length, where the Ball had not 3 Inches to move, and where the Irregularity arising from the Powder unfired ought to have been the most sensible, the Quantity of real Powder collected unfired from a Charge of 12 *dwt.* would have been no more than 16 Grains at a Medium, or $\frac{1}{18}$ of

of the whole Charge; and it being found by Experiment, that the Velocities of Bullets placed in the same Situation vary in the subduplicate Proportion of the Charges, the Deficiency of Velocity arising from the Loss of the $\frac{1}{18}$ of the Charge would be about $\frac{1}{36}$ of the whole Velocity only, which, in the present Case, is not $\frac{2}{10}$ of an Inch in the Chord of the Arch described by the Pendulum measuring the Velocity, and is a less Difference than what frequently occurs in the exactest Repetition of the same Experiments.

Other Circumstances occur, which reduce the Inequality arising from the unfired Powder still lower; but it is thought, that this is fully sufficient to justify the Postulate in Question, especially as, in all Cases of real Use, the Length of the Barrel in proportion to the Quantity of the Charge will be much greater than in the present Instance: Whence the Author presumes, that, in computing the Velocities communicated to Bullets by the Action of Powder, it may be safely supposed, that the whole Charge is fired before the Bullet is sensibly moved from its Place; at least there is no Foundation, from the Experiments made on this Subject by the Committee, to suspect that when small-grained Powder is made use of, any greater Irregularity will arise from the Application of this Supposition, than what would otherwise take place from the Intervention of unavoidable Accidents.

It has been thought necessary to discuss more at large these Two Postulates, because the last of them being almost in the very Words of one of the Questions proposed to be examined by the Committee of
this

this SOCIETY, and having by them been determined in the Negative, those who have not attended to this Subject might suppose, that thereby the Author's Principles were intirely overturned: Now this would be a great Injustice to him, since he has not relied on this Postulate as rigorously true; for he knew, and has himself taken notice in the present Proposition, that some of the Powder escapes unfired; and he has there made some Conjectures on the Cause of it; but, without insisting on the Reality of those Conjectures, he adds, that, " Be that as it may, the Truth of our " Position cannot in general be questioned."

And though it appears from what has been already said, that the Experiments recited by the Committee rather confirm than invalidate the general Sense of that Postulate; yet it is but Justice to own, that they are a full Confutation of the Conjectures of the Author in relation to the Cause why some Part of the Powder comes out unfired; for the Author has supposed, after *Diego Ufano*, that the Part which thus escaped, was scattered in the Barrel, and not rammed up with the rest, or else that it was of a less inflammable Composition: But the Experiments made on this Occasion intirely destroy this Supposition.

As this, or any other Conjecture on the Cause of this Accident, (for it plainly appears not to be for want of Time only) has nothing to do with the general Reasoning of the present Treatise, it is not necessary to enter into it in this Place; but it may not be improper to mention, that, on computing the Quantities of Powder collected from different Charges, one of the Committee was led to conjecture, that what was thus collected was only Parts
of

of Grains that had been fired, but were extinguished by the Blast before they were intirely consumed. This Conjecture is strengthened by the extreme Minuteness of the Particles of all the Powder which was collected, and from the Deficiency of the Saltpetre found in it on Examination: It may be added too, that the Author, by gradually heating a Parcel of Powder, hath set it on Fire, and blown it out again, for at least a Dozen times successively; and he will undertake to repeat the Experiment at any time, if it should be doubted of.

The Postulates hitherto discussed are preparatory to the 7th Proposition. That Proposition is employed in computing the Velocity which would be communicated to a Bullet in a given Piece by a given Charge of Powder, on the Principles hitherto laid down, that is, supposing the Elasticity of fired Powder to be at first 1000 times greater than that of common Air.

In the 9th Proposition these Computations are compared with a great Number of Experiments, made in Barrels of various Lengths, from Seven Inches to Forty-five Inches, and with different Quantities of Powder, from 6 *dwt.* to 36; and the Coincidence between the Theory and these Experiments is very singular, and such as occurs in but few philosophical Subjects of so complicated a Nature.

By this Agreement between the Theory and the Experiments, each Part of the Theory is separately confirmed; for by firing different Quantities of Powder in the same Piece, and in the same Cavity, it appears that the Velocities of the Bullet, thence arising,

arising, are extremely near the subduplicate Proportion of those Quantities of Powder, and this independent of the Length of the Piece: Whence it is confirmed, that the Elasticity of fired Powder in various Circumstances, is nearly as its Density; and this does not only succeed in small Quantities of Powder, and in small Pieces, but in the largest likewise, under proper Restrictions; at least there are Experiments which could not be influenced by this Theory, where the Quantities of Powder were above 100 times greater than what are used by this Author, and in these Trials this Circumstance takes place to sufficient Exactness.

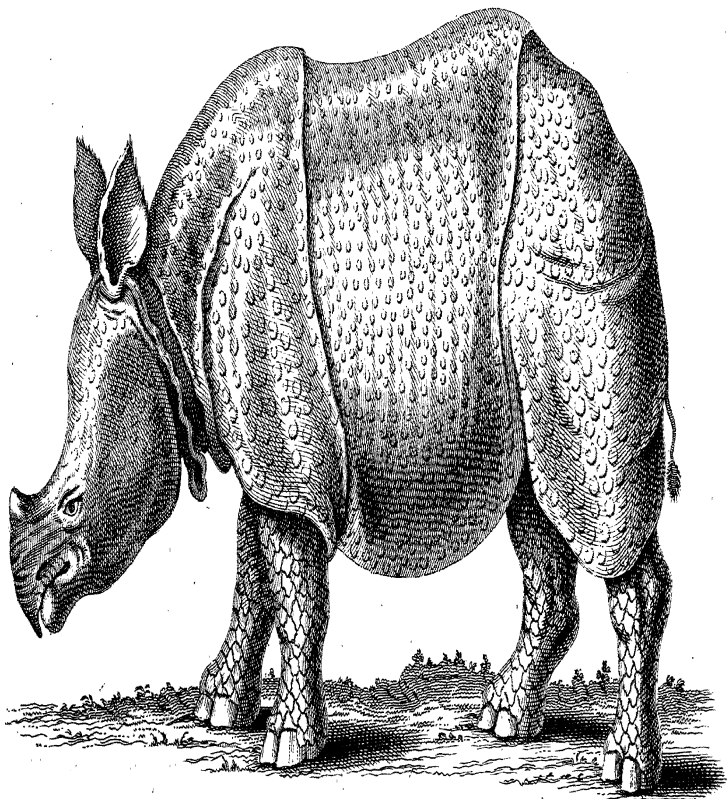
It is presumed then, that by this Theory a near Estimate may be always made of the Velocities communicated to Shells or Bullets by given Charges of Powder; at least these Experiments evince how truly the Velocities of small Bullets are hereby assigned; and the Author can shew by the Experiments of others, that in a Shell of Thirteen Inches Diameter, impelled by a full Charge of Powder, the same Principle nearly holds: It is true indeed, that when the Charge is much smaller than the usual Allotment of Powder, there are some Irregularities, which are particularly mentioned at the End of the 9th Proposition, to which Head too, perhaps, must be referred the Experiments made by the Committee on the Effect of different small Chambers; but in the customary Charges, the Velocities of Bullets resulting from all the Experiments hitherto made, are really such as the Theory laid down in the preceding Part of this Treatise requires. And it appears, that these Velocities are much greater than what they have been hitherto accounted: And there are Reasons from the Theory to believe, that in Can-

non-shot the Velocities may still exceed the present Computation.

The ascertaining the Force of Powder, and thence the Velocities of Bullets impelled by its Explosion, and the assigning a Method of truly determining their actual Velocities from Experiments, are Points from whence every necessary Principle in the Formation or Management of Artillery may be easily deduced: Considering therefore the infinite Import of a well-ordered Artillery to every State, the Author flatters himself, that whatever Judgment may be formed of his Success in these Inquiries, he will not be denied the Merit of having employed his Thoughts and Industry on a Subject, which, though of a most scientific Nature, and of the greatest Consequence to the Public, hath been hitherto almost totally neglected; or, at least, so superficially considered, as to be left in a much more imperfect State than many other philosophical Researches.

With regard to the Second Chapter of this Treatise, relating to the Resistance of the Air, the Author has in his Preface mentioned his Intention of annexing to it a Series of Experiments, on the real Track of Bullets, as modulated by that Resistance: And therefore, as he proposes to complete those Experiments this Summer, unless unforeseen Accidents prevent him, he chooses to postpone any Account of the Subject of the Second Chapter till that time, when he intends to lay the Result of those Experiments before this SOCIETY, in order that any Exceptions or Difficulties relating to them, may be examined and discussed before they are published to the World.

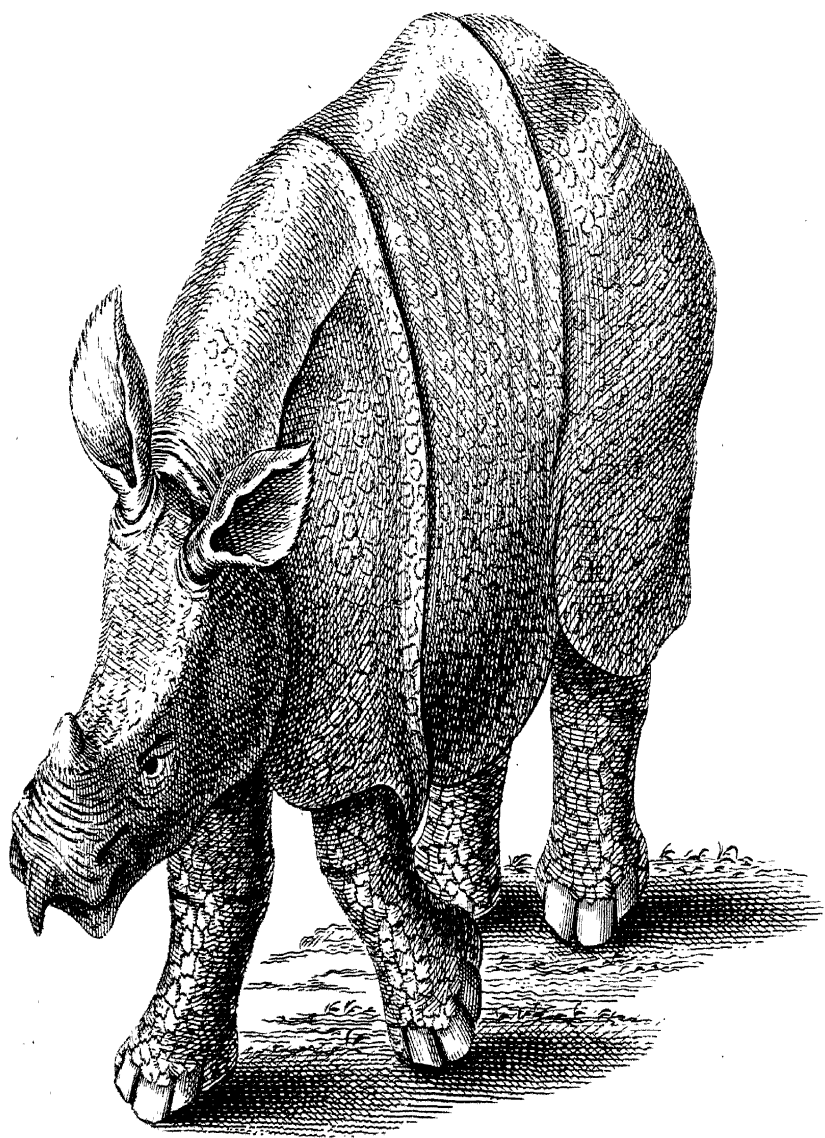
The Reader is desired to correct an Error in the First Paragraph of the 29th Page of the Treatise here referred to, where for *Percussion*, read *Of:il'ation*.



Parlons MD. del.

J. Mynde sc.





Parsons M. del.

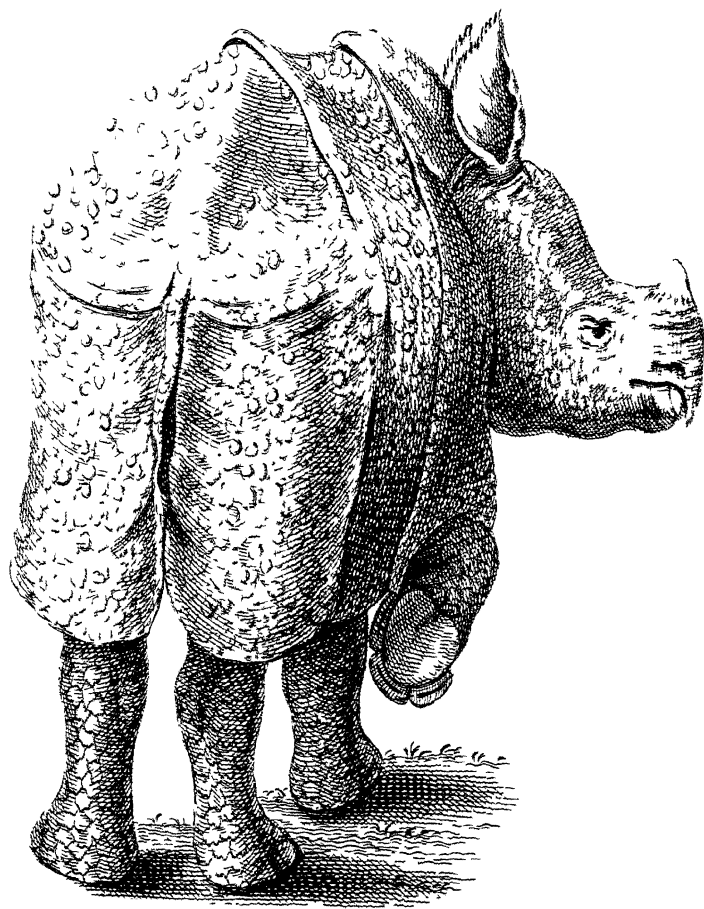




Fig. 5.

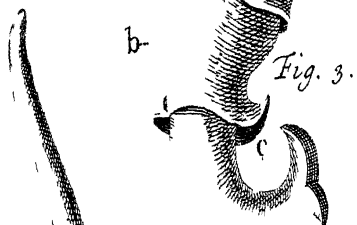


Fig. 3.

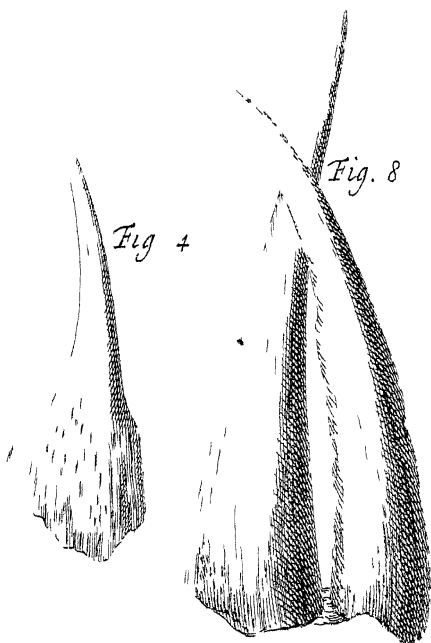


Fig. 4

Fig. 8

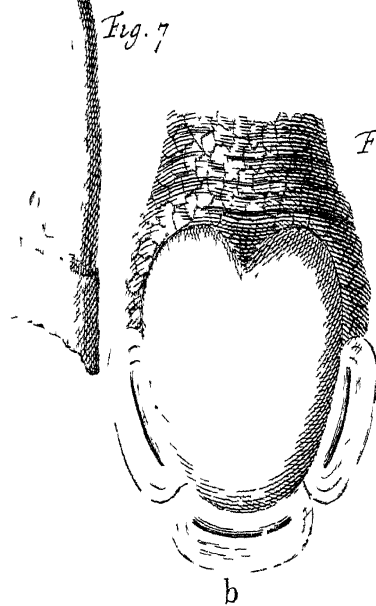
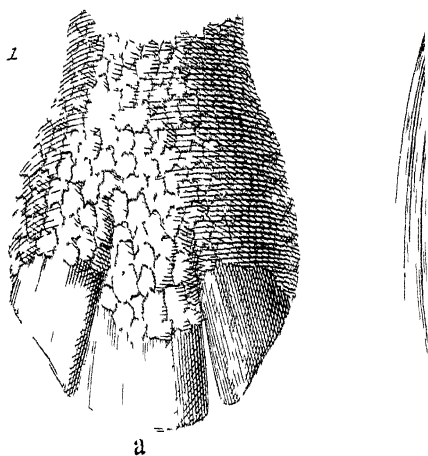


Fig. 7

Fig. 1



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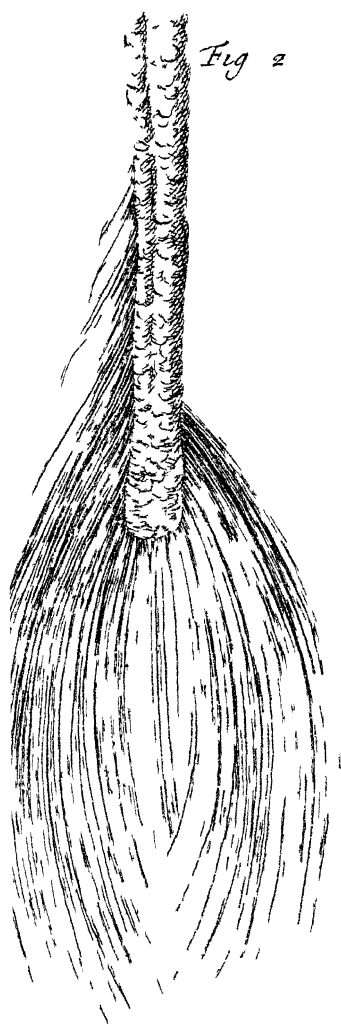


Fig 2

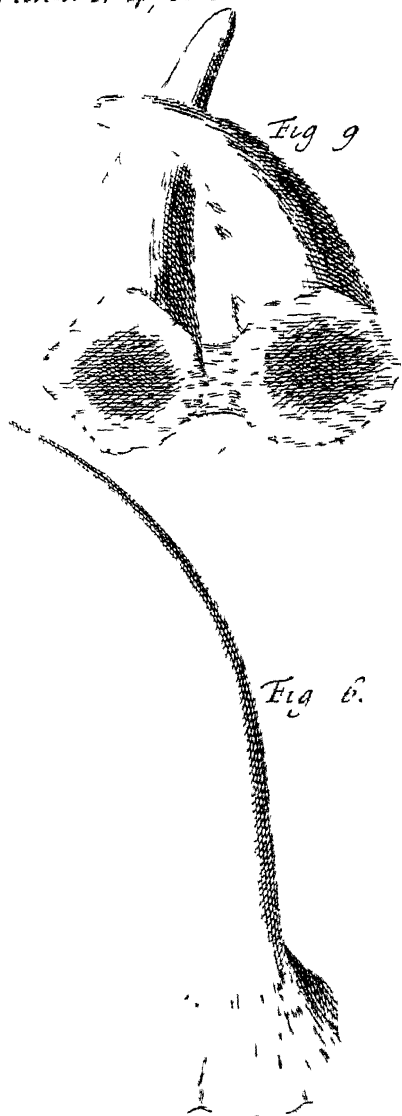
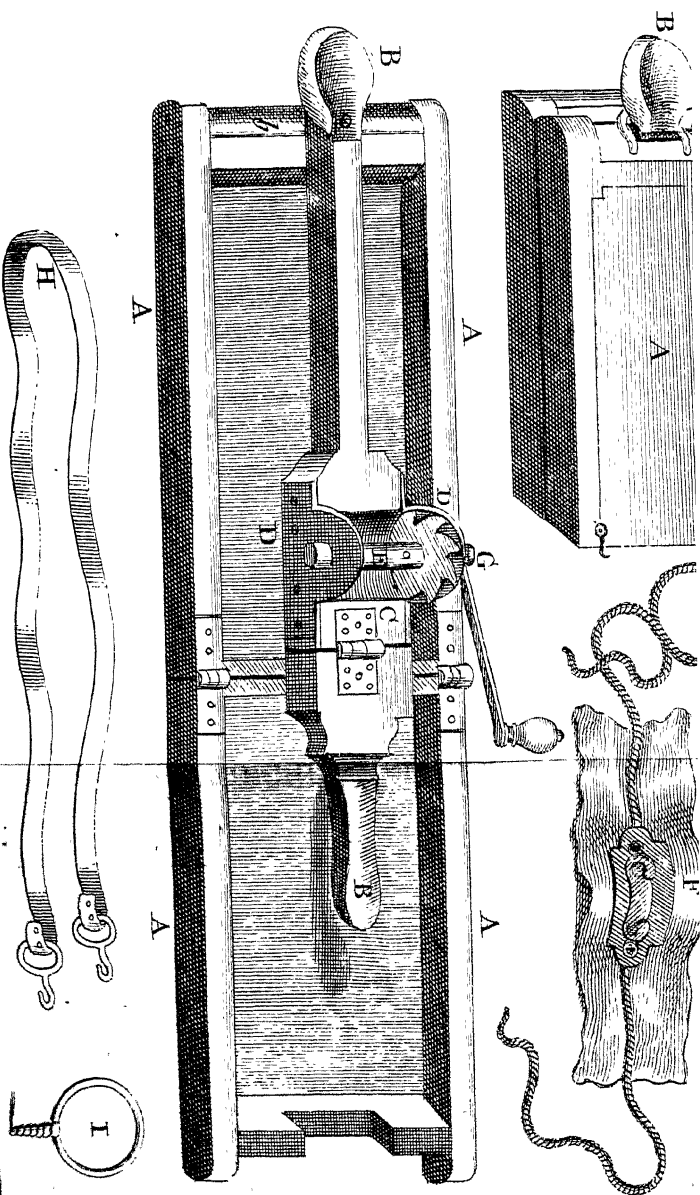


Fig 9

Fig 6.



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Philos Trans N^o 47^o TAB. IV

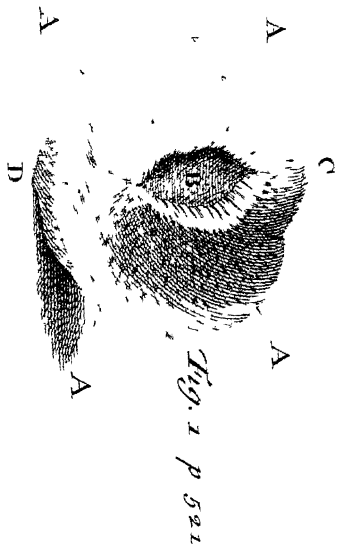


Fig. 1 p. 522

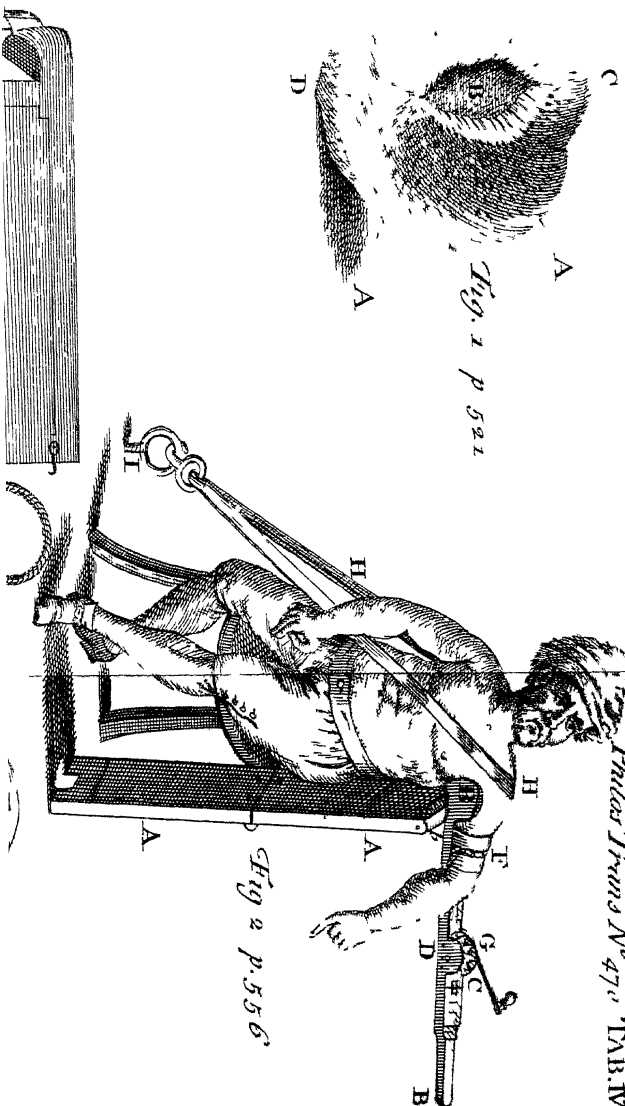


Fig. 2 p. 556

PHILOSOPHICAL TRANSACTIONS.

From *April* 21. to *June* 23. 1743.

The CONTENTS.

- I. *Observationes Cometæ, a R. P. Frantz. Soc. Jef. factæ, mense Februario anni MDCCXLIII. Viennæ Austriæ. Ex Epistola Rev. Petri Isaaci Carnabé, ad Isaacum Lawfon, M. D.* Pag. 457.
- II. *An Abstract of some new Observations upon Insects: By Mr. Charles Bonnet of Geneva. Communicated in a Letter to Sir Hans Sloane, Bart. late President of the ROYAL SOCIETY, &c. Translated from the French by P. H. Z. F. R. S.* 458.
- III. *An Account of an extraordinary Case of the Bones of a Woman growing soft and flexible; communicated to the ROYAL SOCIETY by Mr. Sylvanus Bevan, F. R. S.* 488.
- IV. *Extract of Two Letters from Dr. John Lining, Physician at Charles-Town in South Carolina, to James Jurin, M. D. F. R. S. giving an Account of Statical Experiments made several times in a Day upon himself, for one whole Year, accompanied with Meteorological Observations; to which are sub-joined Six General Tables, deduced from the whole Year's Course.* 491.

The CONTENTS.

- V. *Part of a Letter from his Grace the Duke of Richmond, Lennox and Aubigné, F.R. S. to M. Folkes, Esq; Pr. R. S.* 510.
- VI. *Of the Structure and Diseases of Articulating Cartilages, by William Hunter, Surgeon.* 514.
- VII. *Part of a Letter from the Rev^d Mr. Thomas Lord, to William Folkes, Esq; F.R. S. concerning some Worms whose Parts live after they have been cut asunder.* 522.
- VIII. *A Letter from Ja. Parsons, M. D. F. R. S. to Martin Folkes, Esq; President of the ROYAL SOCIETY, containing the Natural History of the Rhinoceros.* 523.
- IX. *An Account of a Comparison lately made by some Gentlemen of the ROYAL SOCIETY, of the Standard of a Yard, and the several Weights lately made for their Use; with the Original Standards of Measures and Weights in the Exchequer, and some others kept for public Use, at Guild-hall, Founders-hall, the Tower, &c.* 541.
- X. *The Description of an Instrument for reducing a dislocated Shoulder; invented by Mr. John Freke, Surgeon of St. Bartholomew's Hospital, and F. R. S.* 556.
- XI. *A Letter from Peirce Dod, M. D. Fellow of the Royal College of Physicians, London, and Physician to St. Bartholomew's Hospital, to the President of the ROYAL SOCIETY, concerning a Person who made bloody Urine in the Small-Pox, and recovered.* 559.

I. *Observationes Cometæ, a R. P. Frantz Soc.
Jes. factæ, mense Februario anni MDCCXLIII.
Viennæ Austriæ. Ex Epistola Rev. Petri
Isaaci Carnabè, ad Isaacum Lawfon, M.D.*

Read April
21. 1743.

- F**ebr. XI°. (*St. Nov.*) vesperi Cometa
in linea { ϵ & ζ Ursæ Majoris.
recta cum { α & δ Ursæ Majoris.
- XII°. Febr. in linea { γ Ursæ Majoris & λ Draconis.
recta cum { δ & χ Ursæ Majoris.
- XIII°. Febr. Cometa { in linea recta vesperi cum
 { χ Ursæ Majoris & λ Draconis
 { in triangulo rectangulo cum
 { \downarrow & ω Ursæ Majoris.
- XIV°. Febr. in linea recta { α Leonis & ν Ursæ Majoris.
vesperi fere cum { β Leonis & β Virginis.
- XV°. Feb. Vesperi { in linea fere recta cum β Le-
 { onis & β Virginis.
 { in triangulo rectangulo cum
 { ν & ξ Ursæ Majoris.
- XVIII°. Febr. Vesperi in linea recta fere cum β Leonis
& β Virginis; quo tempore Cometa
& β Virginis fere æqualiter distabant
a β Leonis.
- XXI° Febr. Vesperi in Cauda Leonis prope stellulam
sextæ magnitudinis, quæ prope lineam
rectam con- { δ & b Leonis.
stituit cum { β Leonis & π Virg.
Cometa observatus est; neque reliquis
diebus quocumque telescopia detegi
potuit.

Itaque Cometa Viennæ postremum visus est prope prædictam stellam, cujus longit. $13^{\circ} 16' 28''$. 𐄂. & latit. Bor. $17^{\circ} 30'$. Ab hac stella 21 Febr. vesperti hora 8. $8' 22''$. Cometa declinabat versus Septentr. $28' 16''$. occidentalior $1^{\circ} 15' 12''$.

- II. *An Abstract of some new Observations upon Insects: By M. Charles Bonnet of Geneva. Communicated in a Letter to Sir Hans Sloane, Bart. late President of the ROYAL SOCIETY, &c. Translated from the French by P. H. Z. Esq; F. R. S.*

Upon CATERPILLARS.

Read at different times from March 10. to April 28. 1743.

I. **I**T is well known, that among *Caterpillars* there are several Species, which like to live in Society, and which know how to build Nests wherein to shelter themselves against the Injuries of the Air. Of this Sort are those * to which Gardeners have given the Name of *Liverymen*, by reason of the Distribution of their Colours. They may be ranked among the Processioners, or those that follow one another. They all go about, spinning, with great Order: But what is most surprising, is to see them straggle very far from

* *Memoires pour servir à l'Histoire des Insectes.* Tom. I. and II. of the *Paris* Edition.

their Nest, and this often by several Windings and Turnings, without losing their Way. Their Art in doing it deserves notice: It is the same that *Ariadne* made use of to bring *Theseus* out of the Labyrinth in *Crete*: They spin over all the Places where they go. The 1st leads the Way; the 2d follows, spinning; the 3d spins after the 2d and 1st, and so on with the rest. All these Threads form by degrees a small shining Track, a little Path, a *Line* or Two *Lines* broad; and all these Paths meet at the Nest, the Centre, as it were, of all those several Rays.

But to be plainly convinced of the Use of these Threads, let one but break off the Continuation of them in some Place or other, one will see with Astonishment the little *Caterpillars* turn back as at a Loss, without daring to proceed, till one or other, of more Courage than the rest, has restored the Communication, by spinning new Threads.

II. *Caterpillars*, like Men, have particular Tastes (I take the word *Taste* here in its proper Sense): I have observed some, to whom even the Shell of the Egg they were come out of, was agreeable Food. This Fact is not absolutely new. M. *de Reaumur* informs us *, that M. *Maupertuis* has made the like Observation.

But what I have seen more, and which will appear singular, is, that certain *Caterpillars* are not content with gnawing the Shell of the Eggs they came out of themselves, but will gnaw also those of other *Caterpillars* of their own Species, that are near upon hatching.

* Memoires sur les Insectes, Tom. II. p. 165.

Another yet more remarkable Singularity in the Taste of certain Kinds of *Caterpillars*, of the Species of smooth ones, some of the First Class, and others of the Second, is, that they are fond of eating their own *Exuviae*; they have scarcely cast them off but they fall to devouring them. And this will appear still more surprising, if one considers the Condition in which the *Caterpillars* then are. Every one has learnt from *Silkworms*, that, after the moulting, these sort of Insects are extremely weak; and that for a considerable time they remain without any Nourishment, to give time to their new Organs, particularly their Teeth, to strengthen themselves: Yet here you see *Caterpillars*, which, immediately after this critical Operation, greedily devour not only the soft or rather tough Part of their Skin, but even all that is scaly in it, as the Skull, the Legs, &c.—I have even observed some, which seemed to seize upon those, preferably to the rest, and to devour those almost bony Parts, before they fell upon the others, that are much less hard.

III. Nothing surprises more in Insects, than their Industry; and *Caterpillars* yield to none in this respect: Not to speak of those which build for themselves Sheaths or Cases, in which Silk, their own Down, Bits of Bark, Pieces of Paper, &c. are so artfully wrought together; there is one * which builds in Wood, and is able to give to its Case a Hardness greater than that of Wood it self. I shall mention in

* The extraordinary horned *Caterpillar* of the *Willow*, *Memoires sur les Insectes*, Tom. II. p. 264. *seq.* *Goedart.* *Albin.* *Mrs. Merian.* This *Caterpillar* is of the Kind which eat their own Skin.

few Words, how this Insect goes to work : It cuts the Wood with its Teeth, which are very sharp, and severs small Fragments from it, which it binds together with a Silk of a particular Nature, and which seems to differ in several respects from that of other *Caterpillars*; it is properly nothing but a viscous Substance drawn into Threads, which, like Glue, grows hard by degrees.

But, probably, this would not suffice for giving to the whole Work the Solidity that is required, if the industrious *Caterpillar* did not, in some measure, prepare the Fragments of the Wood, before it employs them; and this it does by keeping them in its Mouth for some time, to soak and better fit them for joining themselves into one Body.

This Solidity of the Case of our *Caterpillar* is not what we need further trouble ourselves about; it suffices that the best Care is taken of that Particular: But this *Caterpillar* is also to become a *Butterfly*, and we know, that *Butterflies* have neither Teeth nor Feet to dig withal: How then will this contrive to cut its Way through a Case that is so hard, and so exactly closed up on all Sides? One guesses, perhaps, that it ouses a Liquor which softens that sort of Glue which binds the Bits of the Sawdust together. But what is the Nature of this Liquor? M. *de Reaumur* † has judged, that it must be of a singular Kind. In dissecting some of these *Caterpillars*, I have found near the Mouth, under the *Oesophagus*, a sort of Bladder, of the Bigness of a small Pea, full of a limpid Liquor, and of a penetrating Smell,

† In the Place quoted above.

which I found by divers Trials to be a very active Acid, and which, among other Proprieties it has in common with true Acids, sensibly softens the Glue of the Case. It remains now to shew, that this Liquor is not only of Use to the *Caterpillar*, but is also that very Dissolvent which enables the *Butterflie* to cut its Way through: And this I am not without Hopes of being able to compass.

A Notion adopted by Dr. *Boerhaave* *, that there are no true Acids in Animals, except in the Stomach or Intestines, renders this little Discovery of the more Concern.

IV. We have seen from the foregoing Observation, that *Caterpillars*, though one of those Insects the Structure of which has been most searched into, have yet something still new to present in this respect. And I shall further add, that I have discovered in these Insects a Part of some seeming Consideration, which is a sort of Nipple, or fleshy Protuberance, placed near the Head, under the First Ring; which is commonly concealed in the Inside of the Body, but is forced to shew itself by squeezing the Insect. This Nipple, or Protuberance, which at first I only found single in several *Caterpillars*, I have since met with in others double, and even quadruple; as in that singular horned *Caterpillar* of the *Willow*, which I have already mentioned, and this with some remarkable Varieties. However, they are not all provided with them: I have not found them as yet, for Instance, in those of the First Size, that is to say, the very large ones, nor in those that are very hairy.

* Praxis Medica. Elementa Chem.

But I have observed it in all those *Caterpillars* which, from the Figure and the Stiffness of their Hairs, have been called the *Thorny-ones*. The Use of this Part remains yet unknown to me: All that I know, and that I have learnt by my Experiments, is, that it is not essential to the *Caterpillar*.

V. *Caterpillars* are of those Insects for which one has naturally such an Aversion, that it will easily be believed there are some that have an offensive Smell; and I have actually observed a small Kind of them that smell so like a Bug, that I have thought fit to give them that Name. But what perhaps will appear more strange, is, that there is also a sort of a middling Size, which are smooth, and on the Approach of their Metamorphosis, have a very sweet Rose like Scent; and whose Cases, being made of Earth and of Silk, preserve that Smell for Years together. The *Butterfly* of another *Caterpillar* *, of the middle Size also, but hairy, gave, upon its coming out of its Case, a very sensible Scent of Musk.

Of the FORMICA-LEO.

I. There are few Insects that have been so much and so deservedly admired as the *Formica-Leo*. That excellent Work the *Spéctacle de la Nature* has been so universally read by the Curious, as hardly to let any one be ignorant of its History. However, a little Particular, curious enough, has yet escaped the most diligent Inquiries; and that is the Manner in which he goes to work, when he finds Stones in

* *Memoires sur les Insectes*, Tom. I. Pl. 16. Fig. 8.

his

his Pit, too big to be thrown out with his Horns. Does he then forsake the Place where he settled at first? and does he go somewhere else to set a new Ambulcade? Or, does he remain in his Pit, leaving the Stone there, which he has not been able to remove? Or, after all, does he at last contrive to get rid of it? and what Means does he use to bring this End about? By unwearied observing, I have at last had the Fortune to discover the Secret of his Management. I have seen, that in such Cases the *Formica-Leo* knows how to vary his ways of working: He comes out of the Ground, gets his hinder Parts under the Stone, so that it rests upon his Back, and then by degrees pushes it towards the Top of the Opening, keeping all the while his Poise with great Care. Having thus forced it to the Edge of his Pit, he does not leave it there, for it might roll back again; he therefore pushes it farther off, and then retires to his Pit again.

But sometimes it will happen, that the poor *Formica-Leo* has not the good Fortune to keep the Stone in Poise all the Way; and it rolls back again to the Bottom of the Pit, the Moment it was got to the Brink. This unlucky Accident does not, however, discourage him, but he goes patiently to his Work again, till he gets the Stone out. *Solomon* sends the Slingard to the *Ant*; and we might in like manner send to the *Formica-Leo* those impatient People who give over their Labours upon the first Difficulties they find in them. I have seen some of these Insects, that, after five or six Misfortunes like those I have mentioned, did not yet lose Courage. I have thought I saw the wretched *Myrmica* as condemned

to Hell, in the Poets, rolling a great Stone to the Top of a high Hill, which no sooner was at the Summit, but it slid down again.

The Naturalists will have us admire the Strength of the *Ants*, in transporting their Materials: That of the *Formica-Leo* is doubtless no less worthy of the Attention of all who shall see, as I have done, these little Animals carry to the Brink of their Pit, notwithstanding the Steepness of the Slope, and the Crumbling away of the Earth, Stones Three or Four times as big as themselves.

II. All the *Formica-Leos* that have been hitherto observed, move only backwards; but I have also discovered a Species that move forwards with Activity. These do not, like the others, lie in Ambush for their Prey, but seize on it by mere Force and Dexterity.

Of the PUCERONS, or VINE-GRUBS.

I. The *Pucerons* are pretty well known, so that it will be sufficient to take notice they are that sort of *Gnats*, or *small Flies*, which stick in great Numbers to the Leaves and Stalks of Plants, and cause great Destruction among them. What they present most curious, and which hitherto has been a sort of *Ænigma*, is their way of multiplying. "In every Family of the *Pucerons*, says M. *Reaumur* *, "there are some with Wings, and others without. "According to the usual Analogy, the winged ones

* *Memoires sur les Insectes*, Tom. III. in the Preface, pag. 15.

“ should be the Males, and those without Wings
 “ the Females: But what is a great Singularity in
 “ the History of Insects, is, that here both Sorts are
 “ Females. I have not been able to find out the
 “ Males who impregnate both the one and the other
 “ sort. They all bring forth alive,” &c. Is there
 therefore no Copulation among *Pucerons*? Or are
 they Hermaphrodites like Muscles? In order to
 know this, I tried an Experiment proposed by M.
Reaumur †. I brought up, in perfect Solitude, a
Puceron from the very Instant of its Birth. The
 Expedient I had recourse to for this, was different
 from that which M. *Reaumur* had pointed out. It
 was such as gave me a Facility of observing the little
Puceron at any time, without Fear of letting in
 another. I constantly watched it from Day to Day,
 and from Hour to Hour, for above a Month, usually
 beginning my Observations about Four or Five in the
 Morning, and scarcely discontinuing them till to-
 wards Nine or Ten at Night. I took care to keep
 an exact Journal of its Life, wherein I noted even its
 least Motions, and the most trifling Circumstances.
 At the End of about 12 Days it began to breed, and
 has since brought forth 95 young ones, all alive, and
 most of them under my own Eyes. I have drawn
 up a Table, in which I have marked, with the greatest
 Exactness possible, the Day and the Hour when every
 one of them was brought forth.

I have already repeated this Experiment, Three
 several times, and with equal Success. I have even

brought them up successively in Solitude, as far as the Fourth Generation; and all of them have brought forth.

II. Perhaps one is already from hence inclined to think, that there is in general no Copulation among the *Pucerons*. But there will yet be some room for Surprize, when I say, that I have also observed a Species of them where Copulation does obtain, as it does among so many other Species of Insects or Animals. The Male, like that of the Gall-insects, has Wings, and is a good deal less than the Female. It is, perhaps, one of the most eager Creatures in that respect that is in Nature: I have seen it copulate a great many times in one Day, both with the same Female, and with others.

The ordinary Distinction of the Sex is not the only Singularity I have met with in this Species of *Pucerons*: It has shewn me another no less remarkable. The Females, instead of bringing constantly forth live *Pucerons*, sometimes produce only *Fætuses*, which they lay one alongside of the other, as *Butterflies* do their Eggs.

Besides what relates to Generation, the *Pucerons* have offered me many other curious Particulars. I have seen, for Instance, some, which to cast off their Coats, have given themselves Motions analogous to those of the *Chrysalis* of the *thorny Caterpillar* of the *Nettle*: But to enter into all the Particulars I have met with in these small Insects, would require a Volume.

*Of Insects which are multiplied, as it were,
by Cuttings or Slips.*

M. *Trembley*, a Relation of mine, and an excellent Observer, wrote to me some time ago from the *Hague*, that he had discovered a sort of aquatic Production* of a Nature between a Plant and an Animal; that is to say, which moved, and which had the outward Appearance of a Plant, together with the Property of reproducing what was wanting, after being cut or divided into Two or Three several Parts. So extraordinary a Production could not fail exciting my Curiosity, so much the more, as my Friend did not enter into any Particulars. I spared no Pains to get some of these little Bodies, but all in vain. I only discovered a sort of a long Worm, extremely nimble, upon which I resolved to try the Experiment. As nothing could leave the least Doubt, but that this Insect was truly an Animal, I was assured, that, if my Experiments succeeded, I should fully make out, that there are really Insects to which Nature has given that strange Prerogative of being multiplied, as it were, by Cuttings, and thereby strongly confirm M. *Trembley's* noble Discovery. The Success perfectly answered my Expectation, and I soon had the Pleasure of seeing Two Worms made out of One. But before I enter into farther Particulars, it will perhaps not be amiss to give a slight Idea of the Structure of those Worms. Simple as they seem at their first Appearance, we no sooner examine them with Eyes prepared and armed with Magnifying-glasses, but we discover Parts no less proper to excite and fix our

*. See these *Transactions*, N^o 467. and 469. p. 422.

Attention, than in those Animals we call the most perfect.

I. Their Colour is generally a reddish-brown, or, more exactly, that of the first Peel of an *Onion*. Their Length is about Two or Three Inches; their Thickness that of a common Wire: They are slender, composed of a Series of membranous Rings, continually growing less and less as they approach the Extremities; each of these Rings is furnished in its inferior Part with Four, Five or Six different sorts of whitish Thorns, supplying the want of Legs. Besides these, the Outside of the Worms still presents some other remarkable Particulars, and which afford an agreeable View to the Microscope; these are the Muscles that serve for the Motion of the Rings, and which form an infinite Number of circular Lines or Folds, parallel to each other, which, from the Clearness of the Skin, appear to great Advantage: The Head has not a constant Figure, like that of other Animals; the Insect stretches it, shortens it, enlarges it, and contracts it at Pleasure: Sometimes it shews Two small Elevations one on each Side, which one would think should be the Places of the Two Eyes; what is beyond, terminates in a Point, to make it more easy for the Worm to pierce the Mud. At the Place where the Head is biggest, between the Two Elevations just now mentioned, the Mouth is placed, terminated by Two brown Strokes, which may be compared to the Figure of a Half-moon, or rather that of a reversed Circumflex. When the Insect opens this Mouth, the Opening, which then appears distinctly, is of a circular Shape, and garnished all round with a pretty thick Muscle; it is in great measure this
Muscle,

Muscle, that, by applying itself exactly with its Circumference to a smooth and perpendicular Surface, enables the Insect to make its Way in such Cases. At the other Extremity of the Body, is an oblong Opening, the greater Diameter of which runs parallel to the Length of the Animal, and this gives Passage to the Excrements.

But there is nothing more remarkable than the great Artery in these Worms. This Vessel, which the famous *Malpighi* looked upon as a Chain of Hearts, and which in *Caterpillars*, as well as in many other Insects, extends itself in a strait Line all along the Back, is here more or less folded in different Parts of its Extent; from one End to the other, it is often nothing but Folds and Doublings: Through these crooked Passages, creeps along a Liquor analogous to Blood; from Moment to Moment you may see a Drop of that Liquor, which, setting out from the Extremity of the Tail, runs successively through all those Windings, and at last loses itself in the Brain. It is easy to trace it most Part of its Way, by the alternate Motions of Contraction and Dilation, which are successively excited from Ring to Ring. It seems as if every Part of this Artery, comprehended in the Breadth of one of those Rings, is really a complete Heart, which pushes on, to that which follows next, the Drop of Liquor it has just received from that which precedes it. One can hardly be tired with Admiration of the Appearance which those continual Motions of *Systole* and *Diastole* afford: But the better to perceive it, one should fix one's Eyes upon the Middle of the Body, where the Artery is largest in Diameter; for towards the Two
Ex-

Extremities things are not to be seen so distinctly. Towards the Head, about the Fifth or the Sixth Ring from it, the Artery appears but like a Thread, scarcely discernible, and which, still diminishing continually till near the Mouth, there absolutely ceases to be visible: But what ought most to be taken notice of, is the prodigious Swiftneſs with which the Courſe of the Blood is accelerated in this Place; it ſeems as if it were darted forcibly into the Brain. Towards the Tail, for the Length of ſeveral Lines, it looks as if there was no longer any of the ſame Play; thoſe alternate Contractions and Dilatations, ſo remarkable in the Middle of the Body, here confound themſelves with each other, ſo as to be no longer diſtinguiſhed: In the ſtead of them one only ſees certain Undulations or Layers, as it were, of Clouds, ſucceeding one another with great Regularity.

Under every Junction of the Rings, are to be obſerved ſmall Veſſels with ſeveral Branches, all which ſeem to be Productions of the principal Artery.

All along and immediately under this Artery, is extended the Chanel of the Inteſtines, leſs viſible of itſelf than by the terreſtrial Matters with which it is commonly filled: It is furniſhed, like the Inteſtines of larger Animals, with different Orders of muſcular Fibres, which ſerve to push on, and thruſt out, the Remainder of the Food. If one does not diſcover theſe Fibres by the Eye, one may, at leaſt, know and judge of them by the Effects: One may ſee with Amuſement, how the Excrements are driven on by degrees towards the *Anus*, the Transparency of the Skin, diſcovering eaſily what is under it. However, by
reaſon

reason of the various Motions the Insect gives its Body, these others just described appear for some Space retrograde.

The Earth from which these Worms receive their Nourishment, and which they digest, is not however the only Matter which is admitted into their Bodies; the Air often enters also in Bubbles that are very perceptible. But whereas Fishes have the Air in their Bodies at their own Command, and can make use of it for raising or sinking themselves; our Worms, on the contrary, are, in some measure, mastered by it: As soon as they happen to swallow a certain Quantity of it, it is hardly possible for them, notwithstanding their continual Efforts, to get to the Bottom of the Water; and they are forced to remain on the Surface, till they have got it all out again. I have seen some of these Bubbles alternately driven towards the *Anus*, and repelled towards the Head, for several Minutes together.

These are the principal Particulars, which the Microscope enables us to discover in the Structure of these Worms; which being once known to a certain Degree, we shall, without doubt, the more admire the Wonders of their Reproductions.

II. I mentioned above, that I had divided one of these Worms in two. I put these Two Halves into a sort of Glass Cup, filled only with Water, and attentively watched them during the following Days. I observed that the First Moiety, that which had kept its Head, moved as usual; but what seemed to me far more remarkable, was, that the other Moiety, that had no Head, moved almost as if it had one; it went forwards, resting itself upon the antierour Extremity
of

of its Body; and even made its Way with tolerable Swiftnefs. One could fee, that this was not a Motion without Direction, a Motion produced by a Cause like that which makes the Tail of a *Lizard* move, after it has been severed from the Trunk, but a Motion quite voluntary, the Principle of which seemed not to have been destroyed: One saw it turn aside at the meeting of an Obstacle, stop, and then creep forwards again. When these Two Moieties happened to meet, it was as if they had never composed one and the same Insect; they neither seemed to seek nor to fly each other; each went on its own Way, or, if they went in Company towards the same Place, the First generally outran the Second. But this latter never seemed to shew a sort of Will of its own more plainly, than when I exposed it to the Sun; for then it considerably quickened its Pace.

I had many times Opportunities of admiring the extreme Nicety of the Feeling in these Two Moieties, and especially in the Second. When I approached to it the End of a Splinter, at a time when it was quiet, it seemed to wake, as it were, in a Start, even almost before I had touched it.

Two Days being past, I thought fit to put into the Cup a little Duck-weed and Earth: The First Moiety soon thrust itself among it, but the Second was satisfied with hiding itself among the small Roots of the Weed. I then observed, that, at the Place where it had been cut, there was come out a sort of little Swelling, or Knob, analogous to that which commonly comes out on the Branch of a Tree stript of its Bark. I did not distinguish this so well in the other Moiety; this Knob

seemed to give the Second Moiety more Ease in advancing, and it no longer seemed to be so much affected by all that touched it.

Next Day I took notice, on the Wound of each Moiety, of a small Accretion, distinguishable by the Difference of Colour, which was there much clearer than in the rest of the Body; the following Days it became yet more perceptible. In short, at about a Week's End, each Moiety was again become a complete Worm. The Head that had sprouted out on the Second Part, was, as to its Form, exactly the same with that of the First, and equally fit for all the same Functions. Again, the new Tail of the First was in every respect like the old one. The Heart, the Stomach, &c. had prolonged themselves in one and the other, and the Parts newly produced acted with no less Vigour than the rest; and new Rings had besides been produced successively beyond the old ones.

I took care, from time to time, to measure, with as much Exactness as I could, the Growth of my Two Worms; and I intended to watch them on, with the same Attention; when, at the End of about Eight Days, to my great Surprise, they had found means to escape.

III. This Experiment, which I thus could not pursue as far as I had wished, seeming to require Repetition, I undertook it again, with the same Care: The Success did not fail answering: I soon had the Pleasure to see my Two Moieties recover what they wanted, and become such as they had been before.

IV. I afterwards tried to carry the Division farther, and to divide some of these Worms into Three, Four, Eight,

Eight, Ten, and Fourteen Pieces; and all, or almost all, recovered both Heads and Tails.

In short, to say still more, I cut some of them, even in the midst of Winter, into Twenty-four and Twenty-six Parts: Of the First Division into Twenty-four, there are about Sixteen or Seventeen, full of Life, and most of which begin to complete themselves. Of the Second Division into Twenty-six, there still remain Seven or Eight.

Since my writing what is before, some of these Pieces of Worms have perished, though they had begun to complete themselves. I have Reason to believe, that, when I shall repeat my Experiments in a warmer Season, more of the Pieces will thrive, and become complete Animals: It was proper, however, to try them in Winter, to see the Difference of their Success and Progress. It is worth Notice, that some very small Parts of those Two Worms, one of which was divided into Twenty-four, and the other into Twenty-six Pieces, lived about Three Months, and that in the Winter. For though they were in my Closet, yet the Liquor in *M. Reaumur's* Thermometer did mostly stand between Four and Eight Degrees above Frost, which Degree of Warmth is very inconsiderable; and often, particularly in the Night-time, it was Two or Three Degrees lower.

It is commonly One or Two Days after the Operation in Summer, but about Ten or Twelve in Winter, that the Head and the Tail begin to shoot on those Parts where they were wanting. The Head shews itself first, and lengthens itself continually, for a Week, or more, till it has attained the Length of about a Line and half; and then it ceases to grow. I do

not here mean, that the proper Head has actually that Length; very far from it: But I here give that Name also to Five or Six Rings, which are contiguous to the Head properly so called. It is not so with regard to the Tail, which, having soon surpassed the Head in Length, does not leave off still extending itself; but increases, from Day to Day, so that I do not yet know how far it may go. I shall content myself with saying, that Pieces of those Worms, which, in the Month of *July*, immediately after the Operation, were not quite Two Lines in Length, are at present near Two Inches long: But what may be thought more remarkable, is, that some such Pieces have made in the same time as much Progress, as others Four or Five times as long. I have compared the different Growths of the First Moiety of a Worm about Two Inches long, cut on the 18th of *July*, with those of some of the Pieces of a like Worm cut the same Day into Eight Pieces; and was surprised to find the Quantity of Growth near the same in both Cases. However, it appeared that when the Division was yet carried further, the Pieces thence arising reproduced what they wanted more slowly than the others.

But, if, instead of making this Comparison between the Pieces of different Worms, we make it between those of the same Worm, we shall observe Variations which we perhaps would not have expected. Some of these Pieces will be Twelve or Fifteen Lines long, whilst others will hardly be Four or Five. I have done my utmost to find among those Variations some fixed Point, some Rule, not contradicted by Experience; and it has appeared to me in general, that
the

the Pieces nearest to the Tail are those which make the least Progress. Among this Number is chiefly to be reckoned the last. As to the First, that which keeps the Head, though that is often the Piece which in an equal Time recovers the longest Tail, yet does not this happen so constantly as to build a Rule upon it. My Observations have furnished me with more than one Proof of this. Neither is it a Rule, that all the intermediate Pieces, which have recovered Heads, will also recover Tails: I have Examples to the contrary. But what seems certain, is, that the State of the Worm, the Number of its Divisions, and other Circumstances, seem very much to influence all those Irregularities.

The want of Nourishment, or of such as is proper, may also be a Cause, and that a very natural one, of like Variations. I said above, that those Worms love to be in the Mud, and that they digest it. Those Pieces which I left purposely in clear Water, have usually very well recovered what they wanted to become true Worms; though afterwards they made but little Progress, and almost all successively perished.

V. The learned Dr. *Hales*, in his excellent *Vegetable Staticks*, relates a curious Experiment; by which he proves, that the Bones of Animals, when they are ossified to a certain Degree, do not grow any longer but at their Extremities. Many Observations have convinced me, that it is the same with our Worms. The old Piece, I mean that which was originally cut from the Worm, does not itself lengthen, but its Increase is only owing to the Growth of those additional Parts, that put out at each Extremity.

VI. It is certainly very singular, that the Circulation of the Blood, the Regularity of which appears so essential, yet in certain Insects suffers considerable Changes. Such are those which *Malpighi* has observed in the Silk-worm. And I do not know if it is not as remarkable, that those I am speaking of, have never shewed me any of those Variations, at whatever Time, or in whatever State I have yet observed them, either whilst intire, or when cut into several Pieces. I have constantly, in all these Cases, seen the Liquor that serves them instead of Blood, circulate from the Tail towards the Head, and that in Pieces which were scarcely half a Line in Length, or which, to speak more properly, were only *Granulæ* of Flesh.

I was, by this, able to distinguish the antierour End from the posterour; and to be as sure as possible, that it is always the antierour, on which the Head appears again.

VII. Among those Plants that may be raised from Slips and Cuttings, there are some that seem to have this Property to such a Degree of Perfection, that the least Twig will become a complete Plant again. Hath the great AUTHOR of Nature, when He ordained, that certain Insects, like our Worms, should resemble those Plants in this Particular, allowed them the Power of being reproduced to the same Degree? Or, which is the same thing, will this Reproduction take place in whatever Part the Worms are cut? I have thought this worth inquiring into. In order to it, I cut off from one of these Worms both the Head and the Tail; that is to say, I parted from each of its Extremities a Piece of the Length of
about

about a Line. Both Pieces perished in about Twenty-four Hours, the Tail first, and the Head after. As to the Body, it continued to move almost as if I had not made the Operation. I have even seen, what appeared to me extremely remarkable, that, a few Moments after, it thrust itself into the Mud, making use of its antierior Extremity, as of a Head, to bore its Way through. I have repeated this Experiment with the same Success: So that I am confident I may assert, that there are in the Body of these Worms at least Two Points, where, if they are cut, the Reproduction will not take Place. The one is about the Fifth or Sixth Ring from the Head; the other, at an equal Distance from the Extremity of the Tail. Is not, perhaps, the Condition of the great Artery in these Two Parts the Cause of it? This indeed seems to me probable; remembering, however, that what I have just said only relates to the Two Pieces detached from those Extremities; for, as to the intermediate Body, it not only continues to live, but it is even not long before it regains all that was taken from it. Where then does the Principle of Life reside in such Worms, as, after having their Heads cut off, still shew not only the same Motions, but even the same Inclinations? Yet what is this Difficulty, compared with many others, that at the same time present themselves to our Mind? This wonderful Reproduction of Parts, is it only a natural Consequence of the Laws of Motion? Or does it rather depend on a Chain of minute Buds or Shoots, a sort of little Embryos, already formed, and lodged where the Reproductions are to begin? Are these Worms only mere Machines, or are they like more perfect Ani-

Animals, a sort of Compound, the Springs of whose Motions are actuated by a kind of Soul? And, if they have within themselves such a Principle, how can this Principle afterwards appear in every distinct Piece? Shall we grant, that there are in these Worms as many such Souls as there are Pieces of the same capable of becoming complete Worms? Shall we believe, with *Malpighi*, that these sorts of Worms are all Heart and Brain, from one End to the other? This may be; and yet we know but little the more for it. After all, we must content ourselves with admiring the astonishing Works of the GREAT CREATOR, and sit down in Silence.

VIII. The Nicety of the Sense of Feeling in Spiders has been much talked of; yet do not I know whether our Worms may not, in this Particular also, shew something still more surprising. I have already observed, that upon bringing near them the End of a Splinter, they begin to frisk about, almost before it reaches them: And I have since made other Experiments, which leave me in doubt, whether it is not rather to their Sight than to their quick Sense of Feeling, that I ought to ascribe what I observed in this respect. I have found, that, when the first Rays of the Sun came to fall upon the Vessels of Water in which I kept those Insects, their Motions seemed presently to become more lively. I have fancied, at least, that I saw the same thing, when, after having put them into the Shade, I threw the Light of the Sun upon them from a Looking-glass, or when I observed them by Candle-light: But what seems less liable to Mistake, is, that I have seen some of them creeping about in the Moon-shine, that in the Day-light kept

kept themselves constantly folded together. I would not, however, venture to determine any thing upon this, till I am better satisfied by new Experiments.

• IX. A Twig of *Willow*, *Poplar*, &c. planted in the Earth, takes Root there, and soon becomes a Tree, the least Twig of which will, in its Turn, become another. There is no End of this; and it is the same with our Worms. If we cut those that have been produced by Section, and do not carry the Division at once beyond Twelve or Fifteen Pieces, we shall not fail of having so many Animals. I have had Worms from the Fifteenths, and even the Twenty-fourths, of former Halves and Quarters; and I reckon, that in Two Years time I might, if I would, breed after this manner Forty or Fifty thousand Worms from one single one.

X. But how do these Worms propagate? Are they *viviparous* or *oviparous*? I shall just mention an Observation that to me seemed singular: As I divided one of these Worms into Eight Pieces, I saw some earthy Matter ouising out of one of the Pieces near the Head, in the midst of which I perceived something moving like a whitish Thread. I, at first, made no doubt but it was some Vessel, or like Piece of the Body of the Insect, which, not being quite separated from it, might still draw from thence the Principle of its Motion: But, taking to my Assistance a good Magnifying-glass, I was much surpris'd when I saw, that this supposed Vessel was a small Worm, and exactly of the Figure of that, in the Body of which it had before been inclosed. I immediately resolv'd to bring it up; and, to this End set it apart in a small Vessel filled with Water,

into which I put also a little Earth. It was not long before I was sensible, from the Quickness with which it thrust itself into it, that I had satisfied its Wants: However, from time to time, it came out again, and swam about. I could not but admire the Liveliness of all its Motions; and it was much like one of those little *Eels*, which, by the Microscope, are discovered in Vinegar. I watched it thus above Six Weeks, when, by an unforeseen Accident, I lost it: I was, however, already, in part, informed of what I hoped to learn; I mean, whether this Worm, which I had brought into the World by a sort of *Cæsarean* Operation, would not only continue to live, but would also acquire a greater Length; and this I had seen happen; for the Worm, which at first was hardly a Line in Length, was above as long again, when I had the Accident of losing it. It seems therefore natural to think, that if it had lived longer, it would have been a Worm exactly like that it came from. And I have looked upon this as the more probable, because Thirds of those Worms have also produced others and exactly like themselves.

I have examined some of these little Worms with the Microscope, and observed Two Particularities in them, which I have thought worth Notice: 1st, Long Hairs placed on the Sides of the Body, Two at each Joining of the Rings. 2^{dly}, That the Chanel of the *Intestines*, the great Artery, &c. appeared interrupted for about Two-thirds of the Length of the Body, so that for a Space, which to the Microscope appeared of about Two Lines, the Whole was so transparent, that nothing could be distinguished;

guished; whereas every-where else, except about the Five or Six First Rings, the Parts in Question were plainly visible: And especially the Stomach, by reason of the earthy Substance it was filled with. I have Reason to think, that these small Worms, observed again with fresh Attention, will shew me still something new, in their internal Parts. I divided one on the 28th of *March*, in the Place where I have said that the *Viscera* appeared interrupted. Next Day the Two Pieces buried themselves in the Mud; and on the First of *April*, being both applied to the Microscope, the Latter was found to have already got a Head as well-formed as that of the other Piece, and which had already begun to perform its natural Office of giving Admittance to the Food. It is remarkable, that Worms so tender, and so small, go through the Operation so well, and complete themselves so speedily even in cold Weather. This confirms what I shall observe below, that the more slender these Insects are, the sooner they complete themselves.

This unexpected Observation set me upon examining more carefully the internal Parts of these Worms. With the Help of a good Magnifying-glass, I thought I distinguished, in the Inside of one of the biggest on both Sides of the great Artery, small Worms like those I have spoken of above: I saw them move different Ways, extend themselves, and wriggle about. But, having had recourse to the Microscope, I began to doubt whether that I had before seen was really what it seemed to be. It then appeared, that what I had taken for Worms, were rather the Branches of those Vessels, accompanying the great Artery,

and participating of the Motions of the *Systole* and *Diaſtole* of that Veſſel. Nevertheless, having again reſumed ſeveral times theſe Trials, I have again been perſuaded, I ſaw the ſame Appearances of ſmall living Worms; which makes me ſtill uncertain of the Truth of this Particular, and unable to determine what I ought to think.

XI. We cannot enough admire nor acknowledge the wiſe Conduct of Nature, in the Multiplication of the Species of Animals and Vegetables; ſo far as we ſee, that thoſe which are moſt uſeful to us, commonly multiply, either in a greater Proportion, or may be raiſed with greater Eaſe. But what End could that Wiſdom, which does nothing in vain, have propoſed to itſelf, in granting to ſuch Inſects as theſe a Property and Prerogative, which Animals, far more excellent in our Judgment, ſeem no ways intitled to? It is even certain, that theſe Inſects naturally make uſe of this Power; and it is really true, that the ſame Wonders I have ſeen operated in my Glaſſes, are alſo performed every Day in the Brooks where they live. I have there met with Worms, ſome of which had yet no Heads, and others that only began to recover them. But, which is more, I have found ſome in the ſame State as thoſe which had loſt both their Heads and their Tails, or which had been divided into more than Two Pieces; and all theſe have afterwards fully completed themſelves under my Eyes. Can this therefore be a natural way of multiplying with theſe Inſects? Is it neceſſary, that, in order to bring forth new Worms, their Body ſhould be divided and broke to Pieces?

Or

Or those which I have found divided, were they so only by any Accident? I could hardly have hoped, that my Observations would have furnished me with Answers to these or the like Questions: But Worms of this sort, which I kept intire, having divided themselves of their own Accord, have made me think, that this Accident sometimes proceeds from their having thrust themselves too far into the Earth, or from that Earth's being of too hard and resisting a Nature. It may therefore seem the more fit, that these Insects, whose Bodies are very tender, and liable to be separated, should reproduce what they lost in the manner I have been speaking of. I have farther observed, that they are subject also to a sort of Distemper, analogous to the Gangrene, that sometimes rots off considerable Parts of their Body; which, however, they recover afterwards, like those others which have had the same Parts cut away.

XII. Another sort of Worm, upon which I have begun to make Trials, is also found in the Water. It differs particularly from that I have been speaking of, in that it is considerably thicker. I have divided some of these in the Summer Season into Two, Three, and Four Pieces. Some have recovered the Head and the Tail; but that only after the Space of Twenty Days, during which they always lay like dead. They lived above a Month after, in a State very little different, as to outward Appearance; and afterwards perished, without making any farther Progress. The considerable Difference between the Times in which the Pieces of these last Worms complete themselves, and those employed by the former, with the greater Difficulty in their Success, do they not chiefly pro-

restored from their Thickness? And is it not possibly a Rule, that the slenderer Worms of this Class are, the sooner the Pieces separated from them will resume what is wanting? I should incline to think it is so.

XIII. But if the Water has its Insects, thus produced from Cuttings, the Earth is not absolutely without them. It also contains some perhaps yet more deserving our Admiration, than all that have hitherto been observed in this Kind. Those I mean, are Worms not to be sought for from *Japan* or *China*; but Insects to be met with every-where, and which, in the Eye of the Vulgar, appear the most contemptible. In short, they are only the common Earth-worms. When I began these Inquiries, I judged these Worms more proper than any others to be put to the Trial. Two things persuaded me to it: 1st, Their enormous Size, in comparison of those I had begun to work upon: And, 2^{dly}, The Manner in which they propagate. Every body now knows that these Worms are *Hermaphrodites*, but not such as I have shewn the *Pucerons* to be: That is to say, that an Earth-worm, though it is of both Sexes, cannot ingender without the Concurrence of its like. I have therefore divided some of these into Two, and others into Four Pieces; and some of them, at the End of about Three Months, which they have passed in a sort of Lethargy, did then proceed to resume both Heads and Tails. The Reproduction of the *Anus* is no long Work, a few Days are sufficient for it; but it is otherwise with the Head; that does not seem to perform its Functions in the Pieces of divided Worms, till about Seven Months after the Operation.

Now

Now what further excites my Curiosity, is, to know, whether they will copulate; if they do, the Wonder will be at its highest Pitch. As for what remains, I have made a Remark, not to be here passed over, both upon Earth-worms and Water-insects; which is, that the posteriour Parts always appear to suffer more in the Operation than the anterior. We see the former immediately giving itself, as it were, convulsive Motions, whilst the latter, almost constantly, moves about as usual.

XIV. I have also made Experiments, but without Success, on some sorts of terrestrial Millepedes; likewise on several of those kinds of Worms which metamorphose themselves into *Tipulæ*, or Water-spiders; but no one of them has succeeded.

These are the Observations I have begun to make upon so interesting a Subject. If they are compared with what still remain to be made, they must appear extremely imperfect; and I myself look upon them as no other than a rough Sketch of what others may possibly do hereafter.

Charles Bonnet,

Geneva, March
14. 1742, N. S.

Correspondent of the *Royal*
Academy of Sciences at Paris.

P. S. I should be unpardonable, if I did not endeavour to do all the Justice to Mr. Trembley's Observations that is due to them. He has excellently proved, that the aquatic Production, which he wrote to me about, is a true Animal, and of the Species of the *Polypus*, which, besides the surprising Property of reproducing its Parts, has also another

of multiplying itself by Sprigs or Suckers. Nothing can exceed the Sagacity with which he has conducted those Experiments that have led him into the Knowledge of things so very wonderful and surprising, that they may truly be said to exceed all that Natural History had yet afforded of this Sort.

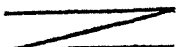
III. *An Account of an extraordinary Case of the Bones of a Woman growing soft and flexible; communicated to the ROYAL SOCIETY by Mr. Sylvanus Bevan, F. R. S.*

Read May 5.
1743.

THE Wife of one B. S. in the Year 1738. was taken with a Diabetes, with the usual Symptoms, *viz.* A frequent and copious Discharge by Urine, a gradual Wasting of the Body, a hectic Fever, with a quick low Pulse, Thirst, great Pains in her Shoulders, Back, and Limbs, and Loss of Appetite. She continued in this manner Two Years, (notwithstanding the Use of Medicines generally prescribed in such Cases) much emaciated; at which time she was attacked with an Intermittent, which soon left her; after which the Diabetes gradually decreased, so that in some few Months she was intirely free from that Disorder, but the Pains in her Limbs still continued. She recovered her Appetite very well, breathed free and easy, and her Hectic very much lessened, though she had some Appearance of it at times.

About Eighteen Months ago, she had such a Weakness and Pains in her Limbs, that it confined her to
her

her Bed altogether; and in a few Months her Bones in her Legs and Arms felt somewhat soft to the Touch, and were so pliable, that they were bent into a Curve; but, for several Months before her Death, they were as limber as a Rag, and would bend any way, with less Difficulty than the muscular Parts of a healthy Person's Leg, without the Interposition of the Bones.

The 12th of *April* 1742. after a long and tedious Illness, she died, near the Age of Forty: And, having the Consent of her Friends, I had the Curiosity to examine more particularly into the several Matters before-mentioned. Upon raising the *Cutis*, I found the *Membrana Adiposa* much thicker than I expected in a Person so much emaciated: The *Sternum* and Ribs, with their Cartilages, were very soft; and all the cartilaginous Parts of the Ribs, at their Articulations, from the *Clavicle* downwards, were doubled over one another on the Left Side, about an Inch, in this Form , only flatter. Upon raising

the *Sternum*, I found the Lungs adhered very close to the Ribs, for Four or Five Inches on each Side; but were more loose and flaccid than usual, and much less in Size: Her Heart was of the common Bigness. Upon viewing her Liver, I found it at least a Third Part bigger than common; and her Spleen was about an Inch and a half in the longest Part, and a Quarter thick: The *Intestines* were very much inflated.

She had Appearances of several *Anchylosses* formed in the small Joints, *viz.* carpal and metacarpal Bones; but, upon laying them open, I found them only like a thin Shell: The cartilaginous *Epiphyses* of the Bones were intirely dissolved, and

no Parts of the Heads of the Bones remaining, but an Outside, not thicker than an Egg-shell.

Upon making Incisions in her Legs and Arms, Five or Six Inches long, I found the outer *Lamina* of the Bones soft, and become perfectly *membranous*, about the Thickness of the *Peritoneum*, containing (instead of a bony Substance) a Fluid of the Consistence of Honey, when it is thick, of a reddish Colour, not at all disagreeable to the Smell: There was no Appearance of any Bones in her Leg and Arms, except near the Joints, which were in part dissolved, and what remained were very soft, and full of Holes, like a Honey-comb: Also the Bones of the Head would easily give way to the Pressure of the Finger.

It is remarkable, that those Parts of the Bones that are the most compact and hard, were first dissolved, while their Heads, which are more spongy and soft, had not so intirely lost their Substance.

When she was in Health, she was Five Feet high, as I am informed by her Husband: I measured her after her Death, and she was but Three Feet Seven Inches in Length, though all her Limbs were stretched out strait, which is Seventeen Inches shorter than she was in her Health: The Bones, which serve as Levers for the Muscles to act upon, being dissolved, these had nothing to keep them extended in their usual Position.

The Person was under the Care of Dr. *Cadwalader* of *Pensylvania*.

IV. *Extracts of Two Letters from Dr. John Lining, Physician at Charles-Town in South-Carolina, to James Jurin, M. D. F. R. S. giving an Account of Statical Experiments made several times in a Day upon himself, for one whole Year, accompanied with Meteorological Observations; to which are subjoined Six General Tables, deduced from the whole Year's Course.*

South-Carolina, Charles-Town, Jan. 22. 1743.

S I R,

Read May 19.
1743.

THAT candid and generous Principle which so universally possesses the Breasts of all true Friends to physical Literature, disposing them to give Assistance and Advice, even to such of the *Illiterati* who shew a Disposition of Inquiry after Truth; and that eminent Character you so justly bear in the Learned World; were sufficient Arguments with me, to lay before you, as a Specimen, one of my *Meteoro-Statical* Tables: The Favour of your Opinion of the Method I have observed, will be most acceptable.

I began these Experiments the First of last *March*, and have continued them ever since, with the Loss only of a few Days; and propose to continue them till the Year is finished, afterwards shall make them a few Days in every Month, and as constantly as possible in epidemic Seasons.

S f f z

What

What first induced me to enter upon this Course, was, that I might experimentally discover the Influences of our different Seasons upon the Human Body; by which I might arrive at some more certain Knowledge of the Causes of our epidemic Diseases, which as regularly return at their stated Seasons, as a good Clock strikes Twelve when the Sun is in the Meridian; and therefore must proceed from some general Cause operating uniformly in the returning different Seasons.

Keil, indeed, has obliged the World with his *Statistical Experiments*, but these his extensive Practice made less perfect than he could have wished, having many deficient Days, and he seldom gives the diurnal Perspiration. Had these been carried on with all the Constancy possible, they could not have so clearly demonstrated the Changes made in the Animal Economy, in the several Seasons, as would a Course of such Experiments made in our Clime, where those Influences are in a much more eminent Degree; and where the Excursions from Heat to Cold are very considerable, and often sudden, I having seen 30 Degrees Difference in 24 Hours by *Fahrenheit's* Thermometer.

Sanctorius, it is true, lived in a warm Climate, and has deduced many useful Aphorisms from his Experiments; but then he has not left us the Experiments themselves: Hence we are not only deprived of the Authorities from whence he deduced his Aphorisms, but likewise of a long-continued Series of Experiments; from whence the Changes induced upon the human Frame, in the different Seasons, might have experimentally appeared.

From

From the Histories of the Air and epidemic Diseases, we learn what Constitutions of the Air are productive of certain Diseases: Were we, however, once furnished with a Course of Statical Experiments of one whole Year, together with the History of the Weather, we, probably, might have more distinct Views of the Nature of the Diseases themselves, by knowing experimentally the Changes produced in our Constitutions, disposing us to such and such Diseases, in certain Periods of the Year.

To these Tables I likewise would have added an Analysis of a little of my own Blood and Urine, in every Month, with the Blood's Cohesion, could I have got the Instruments: But that I propose afterwards to do, if I can get the same Kind which Dr. *Langrish* analysed the Blood, &c. with, and an Instrument exactly the same with his, for measuring the Blood's Cohesion.

The Method I have observed in the Tables is this:

I weigh myself twice every Day, once in the Morning immediately after I rise, and again before I go to Bed at Night. As in *July* 1. my Weight at 6 $\frac{1}{2}$ *a. m.* was *lib.* 165. 13. 0. at 10 in the Night was 167. 5. 4. &c. Twelve Ounces was the Quantity of Urine excreted from 6 $\frac{1}{2}$ in the Morning, to 10 $\frac{1}{2}$ that Night: And 9 $\frac{1}{2}$ Ounces was the Urine from 10 *p. m.* of the First Day, to 7 $\frac{1}{2}$ in the Morning of the Second Day. The Figures placed in the next Column, directly opposite to these Quantities of Urine, express the Quantity perspired in the same Space of Time; *e. g.* 68 Ounces and 3 Drachms was perspired betwixt 6 $\frac{1}{2}$ *a. m.* and 10 $\frac{1}{2}$ *p. m.* in the First Day, and 23 $\frac{1}{2}$ Ounces the Quantity

tity perspired from $10\frac{1}{2} p. m.$ of the First Day, to $10\frac{1}{2} a. m.$ in the Second Day.

The Number of Pulses I take in the Morning, and immediately before I go to Bed at Night.

In the Column titled *Excret. Alv.* the Quantity is in Ounces and Drachms. When the Figures are placed in the upper Part of the Column, that Excretion was in the Morning; when in the middle or lower Part of the Column, then it was in the Middle of the Day, or in the Night before Bed-time. Where 1, 2, or 3, occur in a Column, they express the Number of Stools that Day, as in *July 6.* there were 3 Stools.

The Figures placed in all the rest of the Columns, are in Ounces and Decimals: The Calculations I made with a Two Foot sliding *Gunter's Scale*.

In the Column *Urina 24 horarum*, you have the Urine of 24 Hours calculated each Day; because, as I do not always weigh at one Hour in the Morning, the Space of Time betwixt Two Morning Weighings must be unequal; whence the Difference betwixt the Quantities of each Day does not appear; as from *July 1. 6 $\frac{1}{2} a. m.$* to *July 2d 7 $\frac{1}{2} a. m.$* is 25 Hours, and the Quantity of Urine in that Time amounts to $21\frac{1}{2}$ Ounces, which, calculated to 24 Hours, is 20.62 Ounces. In the same Manner have I calculated the Perspiration of 24 Hours.

In the Column *Urina Diurna 6 Horarum*, is the mean Quantity of Six Hours diurnal Urine calculated; as *July 1. from 6 $\frac{1}{2} a. m.$ to 10 $\frac{1}{2} p. m.$* being 16 Hours, the Quantity of Urine in that Time is 12 Ounces; which, calculated to Six Hours, (upon Suppo-
sition

fition that the Urine was equally secreted in all these Hours, which we know never can be) amounts to 4.50 Ounces.

In the same Manner have I calculated the Nocturnal Urine of Six Hours, and the Diurnal and Nocturnal Perspiration of Six Hours; which serves very well in the following Columns, to shew their Differences, where they are compared together. For the Space of Time in which the Diurnal Urine and Perspiration are excreted, is much greater than that in which the Nocturnal Urine and Perspiration are excreted; whence, without comparing them together, by taking their Means in equal Spaces of Time, their Difference would not appear, as it now does in these Tables at first Inspection.

In the Column *Viginti quatuor Horarum Excreta*, is the whole Quantity excreted in 24 Hours, which is found out by adding together the Stools, and the Urine and Perspiration of 24 Hours by Calculation; whence the exact Quantity retained, or *è contra*, in every 24 Hours, appear in the succeeding Two Columns.

By these tedious Calculations, I have endeavoured, as much as possible, to prepare the Tables for Use, that just Deductions may more easily be drawn from them.

In the Columns *Ciborum Quantit. & Potulentorum Quantit.* the Quantities are in Ounces and Drachms. The Weights I have used are 60 grs = 1 Drachm, 8 Drachms = 1 Ounce, 16 Ounces = one Pound.

The Cloaths in which I dress before I weigh myself are taken care of, so that their Weight shall vary

vary as little as possible in the different Changes of the Air's Humidity.

In the Summer, as Opportunity served, I weighed myself every Hour, Second or Third Hour, through the Day, to investigate the Difference of the Urine and Perspiration, in different Hours of the Day, under different Circumstances; One Table of which I now send you, in which the Urine and Perspiration are likewise in Ounces and Drachms, and is to be read together with the Account of the Quantity of Meat, Drink, and Exercise used; *e. g.*

July 3d, betwixt $11\frac{1}{4}$ and $12\frac{1}{2}$, I drank 20 Ounces of Punch, used no Exercise, was not exposed to the Wind, and was cloathed in a Holland Jacket unbuttoned: Made in that $1\frac{1}{4}$ Hour, One Ounce of flammeous Urine, and sweated so excessively, the Heat of the Air I sat in being 87, that both my Shirt and Jacket being wet with Sweat, was obliged to shift: Whence, though the Perspiration was, no doubt, greatly diminished by the Coldness of the wet Cloaths, towards the End of the $1\frac{1}{4}$ Hour, yet I perspired betwixt $11\frac{1}{4}$ and $12\frac{1}{2}$, $14\frac{5}{8}$ Ounces.—Having shifted, and being cloathed in a Holland Jacket and Chince Gown, was exposed, betwixt $12\frac{3}{4}$ and $2\frac{3}{4}$, to the Third Degree of the Wind's Force; eat $10\frac{5}{8}$ Ounces of roasted Lamb, Bread and Shallots, drank 40 Ounces of Punch, and used no Exercise; in these Two Hours made $3\frac{3}{4}$ Ounces of Urine, and, being exposed to the Wind, perspired only 12 Ounces, though I sweated a little all the Time, and though the natural Heat of the Air was the same as in the former Experiment.—The same Day again, betwixt $2\frac{3}{4}$ and $5\frac{1}{8}$, *p. m.* my Cloathing being the same, and
using

using no Exercise, I drank betwixt 23 and 25 Ounces more of Punch; and the Air being cooled by the Clouds overspreading the Heavens, the Quantity of Urine was greatly increased, amounting in these 2½ Hours to $28\frac{6}{8}$ Ounces; but the Perspiration was so much diminished, that the Quantity of humid Particles attracted by my Skin exceeded the Quantity perspired in these 2½ Hours by $8\frac{1}{8}$ Ounces. Two more Instances of this Attraction you have in the same Table; and, no doubt, it often occurs in the Summer, and might be discovered by any who can conveniently weigh themselves every Second or Third Hour of the Day. Here there was no Waste of the Fluids, the predisponent Cause, according to *Keil*, of such Attraction, but Reason to suspect the contrary, by drinking so plentifully of Punch.

The Punch, or *Diapente*, as I have improperly called it, is made thus: Take Water 2 Pounds, Sugar $1\frac{1}{2}$ Ounce, recent Juice of Limes $2\frac{1}{4}$ Ounces, Rum $3\frac{1}{2}$ Ounces. *M.* This is the Punch we commonly drink in the Summer; but that which we drink in the Fall and Winter is richer, having more Sugar and Rum, and less of the Acid. It is a pleasant, subacid, cooling and exhilarating Drink; and proves an excellent Diaphoretic in warm Weather, and a good Diuretic in cold Weather.

The Barometer is a common portable one; the Diameter of its Bore is about $\frac{1}{2}$ of an Inch.

The Thermometer is *Fahrenheit's*; the other Thermometer is made by *Thomas Heath*, in *London*; and is divided into 90 equal Parts; 65 is the freezing Point, and 49 temperate: I suspect it to be the same with *Hauksby's*, and have called it so in the Tables.

The Hygroscope is a Whip-cord, prepared after the same Manner as that of the SOCIETY's in *Edinburgh*; the Difference betwixt its greatest and least Length, by their Manner of Preparation, I found to be Five Inches; for which I made an Index Five Inches long, and divided it into 100 equal Parts, the First of which is the Hygroscope's greatest Length.

These Instruments are conveniently placed on the Outside of a N. E. Window, in a large square Box, about 3 Feet broad, 6 Feet high, and $1\frac{1}{2}$ Feet deep; which is so constructed, that neither the Sun nor Rain can have Access to the Instruments, and is at the same time sufficiently perforated to shew the Temperature of the Air, having a great Number of large Holes, regularly placed, and passing obliquely upwards, in both Sides, and in the Front, with Weather-boards placed over each Range of Holes, so as to hang over them obliquely downwards; and has likewise a large Window in the Front, which is open from Morning to Bed-time: The Shutters of the Window are in many Places perforated obliquely upwards, that the Air may have a free Circulation through the Box, when the Window is shut at Night.

In the Column *Cæli Facies*, I have only taken Notice of the Sky's Appearance from the *Zenith* to within about 30 Degrees of the Horizon.

N. Nubes. ' Small Rain. ♀. Thunder.

Op. Opaces. " Greater Rain. And

Ob. Obductum. "' Very great Rain.

T. Tennes.

The Characters for Rain express the Time in which it rained, according as they are placed in the Column. When in the upper Part, it rained in the Forenoon:
In

In the middle, Rain about the Middle of the Day: In the under Part, Rain in the Evening, or Night before Bed-time; and when placed upon the Lines which divides the Days, then it rained in the Night.

I have observed the same Rule with the Character of Thunder, in placing the upper Part of it *a* (*2^a*) in the same manner as of the Character of Rain; and likewise have placed it in that Direction, by which the Point of the Compass where the Thunder began, may be known, the Part (*a*) pointing to the Place where the Thunder began, supposing the Points of the Compass to lie in the same manner in the Tables as in Maps. The numerical Figures placed upon its Left-hand, express the Degree, Violence, or Continuance of the Thunder, 4 being the greatest.

Of the Wind's Force, I am obliged to judge by my Senses. Four Degrees of it being insufficient in such Experiments, I have made Eight. For a small Increase of the Wind's Force has a considerable Influence in sweeping away the Heat of our Cloaths; and, thereby cooling the Skin, diminishes Perspiration.

The Depth of the Rain is in Inches and Decimals.

I make Three Observations, by these Instruments, of the Weather every Day, *viz.* in the Morning, and at Bed-time, at the same Hours in which I weigh myself, and the other at Three *p. m.*

Cubiculi Calor is the Heat of the Room where I sleep or sit, by *Fahrenheit's* Thermometer; have mentioned in the *Observationes Miscell.* when I was exposed, in it, to the Wind.

Thus have I now spent near One Year, with no small Labour, Confinement, and Expence in the Loss of Practice, in making these Experiments and

Calculations; and if they will be of any Service to Mankind, of which you are the most proper Judge, shall then obtain all I had in View, in entering upon the Course. I am,

S I R,

Your much obliged,

and very humble Servant,

John Lining.

South-Carolina, Charles-Town, April 11. 1741.

S I R,

*Read May 19.
1743.*

NE A R Three Months ago, I sent you One Table of my *Statical Experiments*, as a Specimen, praying your Opinion of the Method, and if they promised any Helps towards the Advancement of the Medical Art. I will not take up your Time, in giving you the Reasons which first induced me to undertake a Course of such troublesome Experiments for One whole Year, which I have now finished: However, I presume, that a Course of such Experiments, made in a Clime where the Excursions from Heat and Cold, in the different Seasons, are very great, and the Transitions often surprisngly sudden, these Experiments, I say, made almost every Day through the Year, wherein the Day's Urine and Perspiration are distinguished from the Night's, may be of some Use in illustrating the Nature and predisponent Causes of Epidemic Diseases, which so regularly return at stated Seasons; and especially as nothing, I know of, is extant of that
Na-

Nature, so complete as I have endeavoured: But of this, Sir, you are the best Judge.

Lest the Tables I sent you before, should be lost, I have again presumed to trouble you with this; and have sent One Table more of the Experiments, being the remaining Part of *July*, and likewise Six General Tables deduced from the whole Year's Course; these General Tables containing so many Corollaries deduced from the Whole, and exhibiting, at one View, the Changes made in the sensible and insensible Excretions through the whole Year, you may communicate to the ROYAL SOCIETY. All the *Means* in these Tables are calculated after your Method. I am,

S I R,

Your very humble Servant,

John Lining.

N. B. The Table for *July* would have taken up too much Room here: I therefore thought it better to insert only the general Tables, in order to give a general Idea of the whole Year's Observations, which would make a small Volume by themselves. *C. M.*

TABULA Prima

Exhibet Ciborum & Potulentorum quantitatem uncialem & denariam, itemque Excretorum quorumvis summam in diebus omni mense memoratis, in quibus Statica feci Experimenta; unde Incrementum & Diminutio ponderis humani per totum annum abunde patet.

Experiment. Conficiend. Dies.	Cibus.	Potus.	Urina.	Persp.	Excret. Alvin.	Ingesta quam Excreta.	
						Major.	Minora.
Mart.	13	297. 87	1282. 37	971. 50	548. 50	43. 00	17. 25
	12	332. 12	1026. 37	793. 37	532. 37	46. 00	13. 25
April.	13	310. 12	1096. 12	798. 62	591. 62	50. 25	34. 24
	10	244. 75	854. 12	562. 37	506. 00	27. 87	2. 63
Majo	16	424. 25	1293. 37	880. 12	816. 37	61. 00	39. 87
	14	367. 37	1431. 27	804. 37	927. 00	42. 00	25. 00
Junio	14	318. 12	1447. 50	739. 87	1000. 87	52. 75	27. 87
	15	338. 37	1535. 87	780. 87	1069. 50	57. 12	33. 25
Julio	16	378. 37	1787. 75	787. 00	1301. 37	66. 50	11. 25
	15	378. 12	1614. 50	569. 87	1387. 37	55. 75	20. 37
Aug.	15	398. 75	1591. 00	823. 87	1129. 37	50. 37	13. 61
	15	357. 37	1565. 62	828. 37	998. 12	76. 87	0. 62
Sept.	15	350. 00	1599. 25	669. 62	1199. 00	81. 75	1. 12
	15	352. 75	1244. 50	532. 12	1113. 75	52. 25	100. 87
Octo.	16	368. 62	1134. 50	749. 00	642. 75	80. 27	31. 00
	15	373. 75	1123. 87	729. 00	621. 50	110. 12	37. 00
Nov.	15	413. 62	1284. 00	981. 62	609. 37	64. 00	42. 63
	11	284. 75	882. 00	660. 62	442. 75	33. 25	30. 13
Dec.	13	343. 25	1186. 25	875. 12	555. 12	47. 25	52. 01
	14	383. 25	1285. 75	1036. 75	593. 75	53. 75	15. 25
Jan.	15	357. 62	1320. 75	958. 50	629. 75	50. 87	39. 25
	13	304. 75	1328. 37	1069. 50	489. 37	62. 50	11. 75
Febr.	15	382. 00	1381. 87	1138. 75	563. 87	48. 62	12. 63
	13	306. 62	1244. 75	1041. 37	484. 75	41. 50	16. 25

TAB. II.

	<i>Urina</i>			<i>Perspiratio</i>		
	24 Horarum.			24 Horarum.		
	<i>Max.</i>	<i>Min.</i>	<i>Med.</i>	<i>Max.</i>	<i>Min.</i>	<i>Med.</i>
<i>Mart.</i>	102 20	33. 40	70. 59	74. 75	28. 00	43. 23
<i>April.</i>	87. 50	36. 00	59. 17	69 40	34. 00	47. 72
<i>Maius</i>	88. 12	25 25	56. 15	94. 00	30. 62	58. 11
<i>Junius</i>	85. 00	28. 70	52. 09	106. 90	36. 75	71. 39
<i>Julius</i>	92. 90	20. 62	43. 77	105. 00	51. 90	86. 41
<i>August.</i>	76 50	31. 00	55 41	107. 00	38. 90	70 91
<i>Septemb.</i>	78 75	11. 15	40. 06	130. 00	42. 37	77 09
<i>Octob.</i>	73. 40	22. 45	47. 67	63. 10	30. 20	40. 78
<i>Novemb.</i>	99 00	39. 50	63. 16	49. 30	29. 00	40. 47
<i>Decemb.</i>	143. 50	41. 00	70. 81	56. 60	27. 65	42. 55
<i>Jan.</i>	121. 00	39. 75	72. 43	49. 25	33. 10	39. 97
<i>Febr.</i>	115. 00	45. 60	77. 86	46. 10	24. 40	37. 45

TAB. III.

T A B. III.

Quantitates mediæ & Urinæ & Perspirationis tum diurnæ tum nocturnæ, quæ quovis anni mense in paucis temporibus fecernebantur, earumque ad se invicem monstrantur rationes.

	Urina Nocturna 6 hor. est ad Perspir. Noct. 6 hor. ut 1 ad		Urina Diurna 6 horar. est ad Perspir. Diurn. 6 hor. ut 1 ad		Perspir. Noct. 6. hor. est ad Perspir. Diurn. 6 hor. ut 1 ad		Urina Nocturna 6 hor. est ad Urinam Diurnam 6 hor. ut 1 ad.		Perspiratio Nocturn. 6 horar. Media.		Perspir. Diurna 6 horar. Media.		Urina Nocturna 6 horar. Media.		Urina Diurna 6 horar. Media.		
<i>Mart.</i>	18.	35	15.	10	10.	94	9.	90	1.	21	1.	11	0.	59	0.	65	
<i>Aprilis</i>	16.	39	12.	33	13.	03	10.	24	1.	33	1.	27	0.	79	0.	91	
<i>Maius</i>	13.	13	15.	58	16.	44	10.	64	0.	84	1.	54	1.	25	0.	69	
<i>Junius</i>	12.	13	15.	37	20.	69	12.	68	0.	79	1.	63	1.	70	0.	82	
<i>Julius</i>	09.	02	14.	14	26.	73	12.	43	0.	63	2.	15	2.	96	0.	88	
<i>August.</i>	12.	41	17.	02	21.	32	10.	08	0.	73	2.	11	1.	71	0.	59	
<i>Sept.</i>	10.	54	12.	22	22.	58	11.	07	0.	86	2.	04	2.	14	0.	90	
<i>Octob.</i>	10.	33	14.	20	10.	37	9.	88	0.	72	1.	05	1.	00	0.	69	
<i>Nov.</i>	16.	87	13.	88	10.	64	9.	92	1.	21	1.	07	0.	63	0.	71	
<i>Dec.</i>	19.	02	14.	51	12.	03	8.	19	1.	31	1.	47	0.	60	0.	56	
<i>Jan.</i>	21.	16	12.	46	11.	26	8.	22	1.	70	1.	37	0.	53	0.	66	
<i>Febr.</i>	23.	19	13.	31	10.	43	7.	56	1.	74	1.	38	0.	45	0.	57	

T A B. V.

Quonian Ingestorum Quantitas in singulis alterius Tabulæ Columnis mul-
inter se discrepat, adeo ut Excreti cujusvis Incrementum, atque Imminutio, uti
ab aëris temperamento aguntur, haud liquido constet; hæc ideo subducitur
Tabula, ponendo Ingestorum quovis mense vel 30 diebus Quantitatem fuisse
3 543. 40, quæ Medium 30 Dierum 38. 40 exsuperat; unde Incrementum ac
Imminutio Excretorum sensibilem atque insensibilem per totum Annum, sicuti
a Coeli temperie, qualis in TAB. IV. exhibetur, moventur, abunde magis apparet.

[illegible]

TABULA

A prima deducta, Ingestorum ac Excretorum singulis mensibus unlibet mense 30 diebus, eademque Ingestorum & Excretorum diebus tionem quam singula Excreta habent ad Ingesta, Urinaque ad Per- Annum inter se invicem habent, causa pateat apertius, stationes Ba- Min. & Mediæ, cum Aquæ pluviae altitudine unciali & denaria

	Cibus.	Potus.	Ingesta.	Urina.	Perspir.	Excret. Alvin.	Ingesta quam Excreta	
							Maj.	Minor.
Mart.	758. 82	2762. 61	3521. 42	2112. 66	1298. 32	107. 15	03. 29	
Aprilis	724. 92	2545. 91	3270. 83	1765. 00	1442. 12	99. 74		36. 03
Maius	791. 35	2746. 15	3537. 50	1686. 9.	1759. 56	102. 19		11. 19
Junius	679. 21	3086. 76	3765. 9	1573. 51	2141. 86	113. 63		63. 10
Julius	732. 85	3290. 52	4023. 37	1307. 60	2607. 41	118. 10		09. 83
Aug.	756. 12	3856. 62	3912. 74	1662. 23	2127. 49	127. 24		04. 23
Sept.	702. 75	2843. 75	3546. 50	1201. 7.	2312. 75	134. 00		101. 99
Oct.	719. 34	2187. 47	2906. 81	1431. 15	1224. 08	185. 47	66. 07	
Nov.	801. 93	2486. 72	3288. 65	1882. 48	1213. 12	109. 33	83. 72	
Dec.	806. 68	2746. 34	3553. 02	2120. 60	1276. 68	112. 10	43. 64	
Jan.	709. 25	2853. 48	3562. 73	2192. 5.	1194. 40	122. 99	52. 80	
Febr.	735. 80	2818. 12	3553. 92	2240. 3	1123. 20	96. 50		06. 12
Summa								
Total.	8919. 02	33624. 45	42443. 4	21277. 00	19721. 00	1428. 44		

QUARTA.

cialem & denariam Quantitatem complectitur; positis in quohuius ratione, ac ubi facta fuerint Experimenta: Tum proportionem: Deinde ut rationem diversarum, quas hæc per totum rometri Max. & Min. ut & Thermometri & Hygroscopii Max. adjiciuntur.

Urina est ad Perspir. ut i ad Infusa ut i ad	Perspir. est ad Infusa ut i ad	Excret. Alcum sunt ad Infus. ut i ad	Urina est ad Perspir. ut i ad	Barometri Altitudo.		Therm. Fabren. Altitudo.			Hygrosco. Altitudo			Pluviae Quantit
				Max.	Min.	Max	Min	Med.	Max	Min.	Med	
1. 66	2. 71	32. 86	0. 61	30. 40	29. 60	80	34	57	25	4	12	1 141
1. 85	2. 27	32. 89	0. 82	30. 48	29. 58	83	51	67	14	2	7	1 092
2. 15	2. 01	34. 61	1. 04	30. 30	29. 90	87	56	74	30	2	9	5 612
2. 39	1. 75	33. 14	1. 30	30. 28	29. 90	90	66	79	28	5	10	4. 648
3. 07	1. 54	34. 06	1. 99	30. 22	29. 98	91	70	81	30	4	11	3. 012
2. 35	1. 83	30. 75	1. 28	30. 25	29. 95	90	67	77	34	4	12	7. 301
2. 95	1. 53	26. 46	1. 92	30. 36	29. 86	84	56	75	19	6	12	3. 200
2. 03	2. 37	15. 67	0. 85	30. 50	29. 95	73	35	56	33	4	12	1. 257
1. 21	2. 71	30. 08	0. 64	30. 55	29. 73	67	32	52	31	3	14	1. 848
1. 67	2. 77	31. 71	0. 60	30. 58	29. 65	69	21	42	29	3	10	2. 736
1. 62	2. 98	28. 96	0. 55	30. 65	29. 54	63	31	45	40	6	18	4. 492
1. 51	3. 16	36. 82	0. 48	30. 63	29. 88	68	30	46	43	7	16	3 135

39. 475

Vide TAB. V. p. 505.

Medium pondus Matutinum est ad totam Ingestorum unius anni Quantitatem ut 1 ad 15. 97; & ad totum Ingestorum unius mensis ut 1 ad 1. 34.

	℥	3	3	
Jan. 19. . . .	177:00:4	Maximum	} Pondus Matutinum.	
Oct. 1. . . .	159:13:6	Minimum		
	17:02:6	Differentia quidem magna inter pondus Autumnale & Hyemale!		
	168:07:1	Medium pondus Matutinum.		

Urinæ	} totius Anni sunt ad	} 2. 02.		
Perspiratio			} Ingesta ut 1 ad	} 2. 18.
Excreta Alvina				

Perspiratio totius Anni se habet ad Urinam ut 1 ad 1. 08.

Excretiones Alvinae totius Anni se habent ad Urinam & Perspirationem simul sumptas ut 1 ad 28. 7; & ad totum Ciborum totius Anni ut 1 ad 6. 24.

Perspiratio minima Hyemalis per 30 Dies est ad Perspir. Max. Æstivam eodem tempore ut 1 ad 2. 06.

Urina Minima Æstiva per 30 Dies se habet ad Urinam Maximam Hyemalem, eodem tempore ut 1 ad 2. 03.

V. *Part of a Letter from his Grace the Duke of Richmond, Lennox and Aubigné, F. R. S. to M. Folkes, Esq; Pr. R. S.*

Utrecht, Tuesday, ^{May 24}_{June 4}. 1743.

Read June
2. 1743.

— **Y**OU will not be sorry to receive from me some further Account of the *Polypus*; and I must tell you what I have seen in Mr. *Trembley's* Study at *Sorgvliet*. He has there at least a Dozen large Glasses of about a Foot high, each holding a Gallon or Six Quarts of Water, all which are well stocked with those Insects, and he must there have many Hundreds of them. They are, in general, considerably larger than any I had before seen; and as I was first with him on a *Tuesday*, and made him a second Visit on the *Sunday* following, I had the Opportunity of seeing the prodigious Increase they had made in those Five Days. Several single ones that I had left, had in that time put out Five or Six young Ones apiece; and those I had seen him perform Operations upon, were not only recovered, but had most of them produced young ones also. I saw him split the Head of one about Two o'Clock in the Afternoon on *Tuesday*, and, at about Seven the same Evening, each Head eat a small Worm. I saw him split another from the Head to the Tail, and each of those Parts also eat Part of a Worm before Night. Another Operation I saw him make, which I had not before heard of, which was that by putting
one

one of the Points of a very small Pair of sharp Scissars into the Mouth of a *Polypus*, and forcing it out at the very End of the Tail, he then laid it quite open like a *Pigeon*, or a *Barbacute Pig* to be broiled; yet, in about Five Hours, I saw the same *Polypus* with the Parts so reunited again, that I could not perceive any thing had been done to it; and it then eat a large Worm bigger than itself. He then shewed me another odd Particular, which was one *Polypus* that had fairly Two Heads without any Tail; that is, with a Head at each End, as you will see in the First Figure annexed (see p. 513.). This was an accidental Production, and the Manner it came about was as follows: Two young ones grew, as from one Root, out of an old *Polypus*, as in the Second Figure: They both dropt off together, and their Tails not being separated, they appeared as in the First Figure; but, when I saw them, more like the Third, with several young ones putting out from their Sides. Mr. *Trembley* told me, he had seen the like sometimes before, but not often; and that they have then remained Ten or Twelve Days in that Condition, after which they have separated. You may lately have had this from Mr. *Trembley* himself; but, as it was mostly new to me, I would not omit communicating it to you. He had in one of his large Glasses upwards of a Hundred of these Insects all full-grown, and he regaled them all at once before me, with some Thousands of what he calls *des Pucerons d'Eau*, which are small aquatic *Animalcules*, not unlike Fleas, of about the Size of large ones, and which move about with great Swiftnes in the Water.

These

These were no sooner put in, but it was really both a curious and entertaining Sight, to observe in how voracious a Manner not only every *Polypus*, but every young one also that had Arms, though fixed to the Side of its Parent, seized and devoured these *Pucerons*: And as the Body of the *Polypus* is transparent, every one made a very extraordinary Appearance from the Number of *Pucerons* in them; for in several I could very plainly, with my bare Eye, distinguish and count Five or Six of them; and, what was very particular, I could plainly discern some very small black Spots, which I was assured were the Eyes of these *Pucerons*. I had almost forgot to mention to you one extraordinary Observation more of Mr. *Trembley's*, which is, that, in the double-headed *Polypus* of the First and Third Figure, there was at first but one common Gut between them, so that the feeding of one Head had the same Effect as the feeding them both. The Figures are but bare Sketches, but you know I am no Draughtsman, and I think they may be sufficient to inform you of my Meaning. I need not, I believe, tell you with what Satisfaction I passed my Time, and that Mr. *Trembley* is one of the most agreeable Men I have known. He is particularly handy and dextrous in his Operations, and explains himself about them with great Exactness and Perspicuity. He places some Pieces of Packthread cross his Glasses towards the Top: To these some of the Insects fix themselves; and I have seen some that in that Posture have extended their Arms almost to the Bottom, which must have been above Ten Inches. I shall set out from hence in Two or
Three

Three Days, &c. That you may enjoy
the most perfect Health and Satisfaction, is the sincere
Wish of,

Dear SIR,

*Your most faithful
and affectionate Friend, &c.*

**RICHMOND, LENNOX,
and AUBIGNE.**

Fig. 1.



Fig. 2.

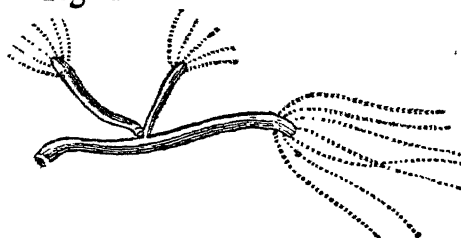
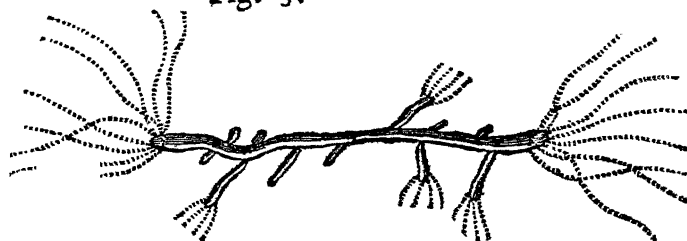


Fig. 3.



X x x

VL Of

VI. *Of the Structure and Diseases of Articulating Cartilages, by William Hunter, Surgeon.*

Read June 2.
1743.

THE Fabric of the Joints in the Human Body is a Subject so much the more entertaining, as it must strike every one that considers it attentively with an Idea of fine mechanical Composition. Where-ever the Motion of one Bone upon another is requisite, there we find an excellent Apparatus for rendering that Motion safe and free: We see, for Instance, the Extremity of one Bone moulded into an orbicular Cavity, to receive the Head of another, in order to afford it an extensive Play. Both are covered with a smooth elastic Crust, to prevent mutual Abrasion; connected with strong Ligaments, to prevent Dislocation; and inclosed in a Bag that contains a proper Fluid deposited there, for lubricating the Two contiguous Surfaces. So much in general.

But if Curiosity lead us a Step further, to examine the Peculiarities of each Articulation, we meet with a Variety of Composition calculated to all the Varieties of Motion requisite in the Human Body. Is the Motion to be free and extensive in one Place? There we find the whole Apparatus contrived accordingly. Ought it to be more confined in another? Here we find it happily limited. In short, as Nature's Intentions are various, her Workmanship is varied accordingly.

These

These are obvious Reflections, and, perhaps, as old as the Inspection of dead Bodies. But modern Anatomists have gone further: They have brought the Articulations, as well as the other Parts of the Body, under a narrow Inquiry, and entered into the minutest Parts of their Composition. The Bones have been traced Fibre after Fibre; but the Cartilages, as far as I can learn, have not hitherto been sufficiently explained. After some fruitless Attempts by macerating and boiling the Cartilages in different *Mensstrua*, I fell upon the Method not only of bringing their fibrous Texture to View, but of tracing the Direction and Arrangement of those Fibres. I shall therefore endeavour to give a short Account of the Structure of articulating Cartilages, and make a few Observations on their Diseases, with a View to advance a rational Explication of their morbid *Phænomena*.

An articulating Cartilage is an elastic Substance uniformly compact, of a white Colour, and somewhat diaphanous, having a smooth polished Surface covered with a Membrane; harder and more brittle than a Ligament, softer and more pliable than a Bone.

When an articulating Cartilage is well prepared, it feels soft, yields to the Touch, but restores itself to its former Equality of Surface when the Pressure is taken off. This Surface, when viewed through a Glass, appears like a Piece of Velvet. If we endeavour to peel the Cartilage off in *Lamellæ*, we find it impracticable; but, if we use a certain degree of Force, it separates from the Bone in small Parcels; and we never find the Edge of the remaining Part oblique, but always perpendicular to the subjacent Surface of the Bone. If we view this Edge through

a Glass, it appears like the Edge of Velvet: a Mass of short and nearly parallel Fibres rising from the Bone, and terminating at the external Surface of the Cartilage: And the Bone itself is planned out into small circular Dimples, where the little Bundles of the cartilaginous Fibres were fixed. Thus we may compare the Texture of a Cartilage to the Pile of Velvet, its Fibres rising up from the Bone, as the silky Threads of that rise from the woven Cloth or *Basis*. In both Substances the short Threads sink and bend in Waves upon being compressed; but, by the Power of Elasticity, recover their perpendicular Bearing, as soon as they are no longer subjected to a compressing Force. If another Comparison was necessary, we might instance the Flower of any corymbiferous Plant, where the *Flosculi* and *Stamina* represent the little Bundles of cartilaginous Fibres; and the *Calyx*, upon which they are planted, bears Analogy to the Bone.

Now these perpendicular Fibres make the greatest Part of the cartilaginous Substance; but without Doubt there are likewise transverse Fibrils which connect them, and make the Whole a solid Body, though these last are not easily seen, because being very tender, they are destroyed in preparing the Cartilage.

We are told by Anatomists, that Cartilages are covered with a Membrane named *Perichondrium*. If they mean the Cartilages of the Ribs, *Larynx*, Ear, &c. there, indeed, such a Membrane is very conspicuous; but the *Perichondrium* of the smooth articulating Cartilages is so fine, and firmly braced upon the Surface, that there is room to doubt whether

ther it has been often demonstrated, or rightly understood. This Membrane, however, I have raised in pretty large Pieces after macerating; and find it to be a Continuation of that fine, smooth Membrane that lines the capsular Ligament, folded over the End of the Bone from where that Ligament is inserted. On the Neck of the Bone, or between the Insertion of the Ligament, and Border of the Cartilage, it is very conspicuous, and may be pulled up with a Pair of Pincers; but where it covers the Cartilage, it coheres to it so closely, that it is not to be traced in the recent Subject without great Care and Delicacy. In this Particular it resembles that Membrane which is common to the Eye-lids and the Fore-part of the Eye-ball, and which is loosely connected with the *Albuginea*, but strongly attached to the *Cornea*.

From this Description it is plain, that every Joint is invested with a Membrane, which forms a complete Bag, and gives a Covering to every thing within the Articulation, in the same Manner as the *Peritonæum* invests not only the *Parietes*, but the Contents of the *Abdomen*.

The Blood-vessels are so small, that they do not admit the red Globules of the Blood; so that they remained in a great measure unknown, till the Art of filling the vascular System with a liquid Wax brought them to Light. Nor even by this Method are we able, in adult Subjects, to demonstrate the Vessels of the true cartilaginous Substance; the Fat, Glands, and Ligaments, shall be red with injected Vessels, while not one coloured Speck appears upon the Cartilage itself. In very young Subjects, after a subtle Injection, they are very obvious; and I have
found

found their Course to be as follows: All round the Neck of the Bone there are a great Number of Arteries and Veins, which ramify into smaller Branches, and communicate with one another by frequent *Anastomoses*, like those of the Mesentery. This might be called the *Circulus Articuli Vasculosus*, the vascular Border of the Joint. The small Branches divide into still smaller ones upon the adjoining Surface, in their Progress towards the Centre of the Cartilage. We are very seldom able to trace them into its Substance, because they terminate abruptly at the Edge of the Cartilage, like the Vessels on the *Albuginea Oculi* when they come to the *Cornea*. The larger Vessels, which compose the vascular Circle, plunge in by a great Number of small Holes, and disperse themselves into Branches between the Cartilage and Bone. From these again there arises a Crop of small short Twigs, that shoot towards the outer Surface; and whether they serve for nourishing only, or if they pour out a dewy Fluid, I shall not pretend to determine. However that be, I cannot help observing, that the Distribution of the Blood-vessels to the articulating Cartilages is very peculiar, and seems calculated for obviating great Inconveniences. Had they run on the outer Surface, the Pressure and Motion of the Two Cartilages must infallibly have occasioned frequent Obstructions, Inflammations, &c. which would soon have rendered our Motions painful, and at last intirely deprived us of them. But by creeping round the cartilaginous Brim, where there is little Friction, or under the Cartilage, where there is none, they are perfectly well defended from such Accidents.

It

It were to be wished we could trace the Nerves of Cartilages: But, in relation to these Organs, here, as in many other Parts of the Body, we are under a Necessity, from the Imperfection of our Senses, of being satisfied with mere Conjecture. And though, from the great Insensibility of a Cartilage, some have doubted of its being furnished with Nerves; yet, as it is generally allowed, that these are a *sine qua non* in the Growth and Nourishment of Animals, we have no sufficient Reason to deny their Existence in this particular Part. With regard to the manner of their Distribution, we may presume, from Analogy, that they follow the same Course with the Blood-vessels.

The articulating Cartilages are most happily contrived to all Purposes of Motion in those Parts. By their uniform Surface, they move upon one another with Ease: By their soft, smooth, and slippery Surface, mutual Abrasion is prevented: By their Flexibility, the contiguous Surfaces are constantly adapted to each other, and the Friction diffused equally over the Whole: By their Elasticity, the Violence of any Shock, which may happen in running, jumping, &c. is broken and gradually spent; which must have been extremely pernicious, if the hard Surfaces of Bones had been immediately contiguous. As the Course of the cartilaginous Fibres appears calculated chiefly for this last Advantage, to illustrate it, we need only reflect upon the soft undulatory Motion of Coaches, which Mechanics want to procure by Springs; or upon the Difference betwixt riding a Chamber Horse and a real one. To conclude, the Insensibility of articulating Cartilages is wisely contrived, as by this means

means the necessary Motions of the Body are performed without Pain.

If we consult the standard Chirurgical Writers from *Hippocrates* down to the present Age, we shall find, that an ulcerated Cartilage is universally allowed to be a very troublesome Disease; that it admits of a Cure with more Difficulty than a carious Bone; and that, when destroyed, it is never recovered. *Hildanus*, in considering these Diseases, has observed, that when the Cartilages of a Joint were destroyed, the Bones commonly threw out a cementing *Callus*; and thus a bony *Anchylosis*, or immoveable Continuity, was formed where the moveable Joint had been. So far as I have had Opportunities of examining diseased Joints, either after Death or Amputation, I have found, according to the Nature and Stage of the Disease, the Cartilages in some Parts redish and lax; or soft and spongy; or raised up in Blisters from the Bone; or quite eroded, and, perhaps, the Extremities of the Bones carious; or, lastly, a bony *Anchylosis* formed. But I could never see, nor indeed hear of, the least Appearance of an Exfoliation from the Surface of the Cartilage. Now, if we compare the Texture and morbid *Phænomena* of those Cartilages together, all the diseased Appearances will admit of as rational a Solution, as perhaps any other Part of the vitiated Oeconomy.

It appears from Maceration, that the transverse Fibrils are extremely tender and dissoluble; and that the Cohesion of the Parts of the strait Fibres is stronger than their Cohesion with the Bone. When a Cartilage therefore is inflamed, and soaked in purulent Matter,

Matter, the transverse or connecting Fibres will the soonest give way, and the Cartilage becomes more or less red and soft, &c. If the Disorder goes on a little longer, the Cartilage does not throw off a Slough, but separates from the Bone, where the Force of Cohesion is least, and where the Disease soon arrives, by reason of the Thinness of the Cartilage. When the Bone is thus exposed, the Matter of the Ulcer, or Motion of the Joint, corrodes or abrades the bony Fibres. If the Constitution is good, these will shoot forth a *Callus*; which either cements the opposite Bones of the Articulation, or fills up the Cavity of the Joint, and for the future prevents Motion. But if, unfortunately, the Patient labours under a bad Habit of Body, the Malignancy, having got Root in the Bone, will daily gain ground, the *Carries* will spread, and at last the unhappy Person must submit to Extirpation, a doubtful Remedy, or wear out a painful, though probably a short Life.

Explication of the FIGURE.

- Figure 1. TAB. IV. *Represents a View of the Patella on the Backside, where it is covered with a smooth Cartilage. In this we may observe,*
AAAA. *The Surface of the Cartilage, appearing, when the Perichondrium is removed, like Velvet. Near the Middle, Part of the Cartilage is taken out, in order to shew*
B. *The subjacent Surface of the Bone: And*
C. *The Thickness of the Cartilage, where the perpendicular Fibres are seen very distinctly.*
D. *The scabrous lower Point of this Bone, into which the Ligament is inserted that binds it to the Tibia.*

VII. *Part of a Letter from the Rev^d Mr. Thomas Lord, to William Folkes, Esq; F. R. S. concerning some Worms whose Parts live after they have been cut asunder.*

Read June 9.
1743.

— AFTER I had, without Success, made several repeated Searches for the *Polypus*, in several Fishponds, and a small Stream in my Parish, I applied myself to collect the different Insects of various Sorts I had there met with, and which were of more than 30 Kinds, all which I put together; but some of them voraciously seized upon others, and devoured them, so that in a Day's time I had hardly any left, but a few of one Sort, which rolled themselves up like *Millepedes*, or *Hog-tice*, but were, upon the Whole, more of the *Leech* Kind, and could extend themselves about an Inch in Length. These I cut asunder, but the Pieces died in about 30 Hours after the Operation. I then recollected, that, in the Account * published by Dr. *Mortimer*, mention is made of a *French* Gentleman, that had discovered *Water-worms*, that would live after cutting: I searched for all I could find fastened either upon rotten Wood, Leaves, Straws, or Stones, that I took out from the Bottom of the Water, and cut of every Sort asunder; but none lived above 48 Hours, except these I here send you. In one Glass are Four Pieces that now seem to be complete Worms, and the same as the Two in the other Phial: These Four Pieces, 12 Days since, were Two Worms: I cut them asunder with my

* See these *Transactions*, N^o 467.

Penknife, and found that each Part, from the First, continued vigorous and strong; and I could, by my naked Eye only, see that in Three Days the Ends where the Wounds were given, were grown sharper, and that they moved along like the intire Worms. I am, &c.

Wheltham in Suff.

June 1. 1743.

Thomas Lord.

The Two intire Worms here mentioned to have been contained in one of the Phials sent up by Mr. Lord, were each cut presently after into Two Pieces, which soon after completed themselves, grew longer, and were several Weeks after in a vigorous and thriving Condition.

VIII. *A Letter from Dr. Parsons to Martin Folkes, Esq; President of the ROYAL SOCIETY, containing the Natural History of the Rhinoceros.*

SIR,

Read June 9.
1743.

ALthough many Authors have given Accounts and Figures of the *Rhinoceros* from time to time, and although there was one in *England* in 1685. yet how far were we from having the least Notion of his Form, when we came to see him in 1739. It was not difficult, even before the Arrival of the latter here, to discern an Uncertainty in the Figures that were exhibited of that Animal, because they differed so widely from each

Y y y 2

other;

other; and, as there was such a Variety in them as might induce one to take them for different Animals, there was no knowing where to fix. This will fully appear in viewing the Collection I have the Honour to lay before you.

Albert Durer's Figure of this Creature has led several of those Natural Historians, that have wrote since his Time, into Errors; for such have always copied him; and indeed many have exceeded him in adorning their Figures with Scales, Scallops, and other fictitious Forms. Now, from the Badness of his Figure, I am induced to believe that great Man never saw the Animal; for he certainly could not have been so mistaken in the Performance. However, from the strictest Inquiry I was capable of making, it seems most probable, that a Sketch was sent to him from *Portugal*, by a Person who took it from a *Rhinoceros*, which was sent from the *East-Indies* to *Emanuel* King of *Portugal*, as a Present; and that *Albert* improved and embellished it into the original Drawing, which is in Sir *Hans Sloan's Museum*. The Inscription, in *German*, written under this Drawing, proves it very clearly, of which the following is a close Translation.

“ In the Year 1513. upon the 1. Day of *May*, there
 “ was brought to our King at *Lisbon* such a living
 “ Beast from the *East-Indies* that is called *Rhino-*
 “ *cerate*: Therefore on account of its Wonderfulness
 “ I thought myself obliged to send you the Repre-
 “ sentation of it. It hath the Colour of a Toad,
 “ and is close covered over with thick Scales. It is
 “ in Size like an *Elephant*, but lower, and is the *Ele-*
 “ *phant's* deadly Enemy; it hath on the fore Part of
 “ its

“ its Nose a strong sharp Horn; and, when this Beast
 “ comes near the *Elephant* to fight with him, he
 “ always first whets his Horn upon the Stones; and
 “ runs at the *Elephant* with his Head between his fore
 “ Legs; then rips up the *Elephant* where he hath
 “ the thinnest Skin, and so gores him: The *Elephant*
 “ is terribly afraid of the *Rhinocerate*; for he gores
 “ him always, where-ever he meets an *Elephant*; for
 “ he is well armed, and is very alert and nimble.
 “ This Beast is called *Rhinocero*, in *Greek* and *Latin*;
 “ but, in *Indian*, *Gomda*.”

The first Print published by *Albert Durer* himself
 has a *German* Inscription over it, somewhat differing
 from the manuscript one, of which the following
 is likewise an exact Translation, with this Date and
 Mark, thus :

1515
RHINOCERUS



“ In the Year 1513 from the Birth of Christ, upon
 “ the 1st Day of *May*, there was brought to the most
 “ potent *Emanuel* King of *Portugal* at *Lisbon*, from
 “ *India*, such a living Beast. They call it a *Rhino-*
 “ *cerus*: It is here represented in all its Shape. It
 “ has a Colour like a speckled *Tortoise*, and is very
 “ closely covered over with thick Scales; and is in
 “ Size as an *Elephant*, only of shorter Legs, and very
 “ well armed. It has a sharp strong Horn forwards
 “ upon his Nose, which it begins to whet when it is
 “ near a Rock; therefore it is a conquering Beast, and
 “ the

“ the *Elephant*’s deadly Enemy: The *Elephant* is
 “ greatly afraid of him; for, when he meets with him,
 “ the Beast runs at him with its Head between his
 “ fore Legs, and rips up the *Elephant*’s Belly, and
 “ kills him; for he cannot get rid of him: Besides, the
 “ Beast is so armed, that the *Elephant* can do nothing
 “ to him: They say likewise, that the *Rhinocerus* is
 “ swift, alert, and cunning.”

Many Years after this, one *Hendrik Hondius* published in *Holland* an exact Copy of *Durer*’s Print, counterfeiting the Date and Mark; but gives an Inscription in *Low Dutch*, nearly the same as that under the original Print.

Bontius * says, he has often seen these Animals in the Woods and Stables abroad, and values himself for having exhibited a Figure without the Decorations that *Albert Durer* put upon his; and yet, instead of the Hoofs which are proper to the Animal, he has drawn a *Paw* not unlike that of a Dog, only something bulky.

The Figure given by *Chardin* in his Voyages has some Truth, as to the Folds or *Plicæ* in the Skin of the *Rhinoceros*; and likewise as to the Feet: But in other respects it is not like the Animal. There is also a little Truth in the Figures of *Camerarius*; see his Emblems taken from Animals; but far from a thorough Representation of the Creature: And, in short, the other Originals, as that taken from the *Rhinoceros* in 1685. that published by *Carwitham*

* *Bontius* calls this Animal *Abada*, which probably may be the *Javan* Name.

in 1739. and, to look back to the *Roman Times*, those in the Pavement of *Præneste*, and *Domitian's Medals*, are very inaccurate, but have none of *Albert Durer's Decorations*.

When that *Rhinoceros* arrived here in 1739. Dr. *Douglas*, who let slip no Opportunity of improving Natural Knowledge, intended reforming the History of him, and therefore went frequently to see him; and, on *June 24.* of this Year, exhibited before the ROYAL SOCIETY a Drawing of the same *Rhinoceros*, with a Collection of Figures of that Creature, taken from several Authors, who had wrote of him before. He mentioned also his Dimensions; and, on the 28th of the same Month, he produced a Collection of Horns, with some Account of them, but proceeded no farther. Since therefore another Occasion may not offer in many Years, and that there is no Place more proper for recording Truth in Natural History, than in the *Transactions* of this Learned SOCIETY, I have the Honour to entertain them, in Obedience to your Commands, with the following Account of the Male *Rhinoceros* that was shewed in *Eagle-street* near *Red-Lyon-Square*, in 1739. and the Drawings annexed to it, which I had drawn up at that Time, and put among some curious Physico-medical Miscellanies I have collected, and illustrated with Drawings, in order one Day to be published.

In this Account I have had no Regard to those of other Authors, but have barely described him, as I have often seen him on purpose, both in the above-mentioned Place, and a long time after, when he was shewed at a Booth near the *London-Spaw*.

The Drawings annexed to the *Transactions* (Plate I. and II.) are a side, fore and back View of the Animal fore-shortened*; all which Attitudes I the rather chose, as they will convey to Posterity a clear *Idea* of him, and as the *Drawings*, and Two *Pictures*, (one of which is in Dr. *Mead's Museum*) were all *Profils*, that I had done before. The other Drawings, (Plate III.) joined to these, are the Figures of Two single Horns; and a double one or Two sticking to the same Piece of Skin; the *Penis*; the Tail of an old *Rhinoceros*; and an upper and under View of one of the Feet, pretty large; which shall be all more fully mentioned in the Table of *References*, having omitted nothing that I thought could serve to the better Illustration of this wonderful Creature.

Humphry Cole, Esq; being Chief of the Factory at *Patna* in *Bengal*, procured this *Rhinoceros*, when young, and sent it to *England* by Captain *Atton* in the Ship *Lyel*, which arrived on the First of *June* 1739. The *Rhinoceros* was brought to *Eagle-street*, *Red-Lion-Square*, on the 15th of the same Month; and it was said by those who took care of him, that from his being first taken, to the time of his landing in *England*, his Expences amounted to One Thousand Pounds Sterling.

He was fed here with Rice, Sugar, and Hay: Of the first he eat Seven Pounds to about Three Pounds of the Sugar; they were mixed together, and he eat

* At first I designed only Two Views, the fore and back, of the Animal, for this *Transaction*; but as you justly think the Account will be the more perfect for having a *Profil* also added, I have obeyed your Commands, Sir, in making that View,

this Quantity every Day, divided into Three Meals, and about a Truss of Hay in a Week, besides *Greens* of different Kinds, which were often brought to him, and of which he seemed fonder than of his dry Victuals; and drank large Quantities of Water at a Time, being then, as I was informed by his Keeper, Two Years old.

He appeared very peaceable in his Temper; for he bore to be handled in any Part of his Body; but is outrageous when struck or hungry, and is pacified in either Case only by giving him Victuals. In his Outrage he jumps about, and springs to an incredible Height, driving his Head against the Walls of the Place with great Fury and Quickness, notwithstanding his lumpish Aspect: This I have seen several times, especially in a Morning, before his Rice and Sugar was given him; which induces me to believe he is quite indomitable and untractable, and must certainly run too fast for a Man on foot to escape him.

As to his Size, he did not exceed a young Heifer in Height; but was very broad and thick. His Head, in Proportion, is very large, having the hinder Part, next his Ears, extremely high, in proportion to the rest of his Face, which is flat, and sinks down suddenly forward towards the Middle, rising again to the Horn, but in a lesser Degree. The Horn stands on the Nose of the Animal, as upon a Hill. I have seen the Bones of a Head of one of these, in Sir *Hans Sloane's Museum*; and the Part on which the Horn is fixed, rises into a blunt Cone, to answer to a Cavity in the Basis of the Horn, which is very hard and solid, having no manner of Hollow nor Core, like

those of other Quadrupeds. That of this Animal, being young, does not rise from its rough Basis above an Inch high, is black and smooth at the Top, like those of the Ox-kind, but rugged downwards; the Determination of its Growth is backwards, instead of strait up; which is apparent, as well in the different Horns of old *Rhinoceros's*, which I have seen, as in this of our present Subject; for the Distance from the Basis to the Apex of this, backward, is not within a third Part so long as that before, and it has a curved Direction; and, considering the Proportion of this Animal's Size to its Horn, we may justly imagine, that the Creature which bore any one of those great ones that I have seen, must have been a stupendous Animal in Size and Strength; and, indeed, it were no Wonder, if such were untractable at any rate.

The Sides of his under Jaw are wide asunder, slanting outward to the lower Edge; and backward to the Neck, the Edges turn outward: From this Structure his Head naturally looks large.

That Part that reaches from the fore Part of the Horn towards the upper Lip, may be called the Nose, being very bulky, and having a kind of circular Sweep downwards towards the Nostrils: On all this Part he has a great Number of *Rugæ* running cross the Front of it, and advancing on each Side towards his Eyes.

The Nostrils are situated very low, in the same Direction with the *Rictus Oris*, and not above an Inch from it. If we look at him in a fore View, the whole Nose, from the Top of the Horn to the
Bottom

Bottom of his lower Lip, seems shaped like a Bell, *vis.* small and narrow at Top, with a broad Basis.

His under Lip is like that of an Ox, but the upper more-like that of a Horse; using it, as that Creature does, to gather the Hay from the Rack, or Grass from the Ground; with this Difference, that the *Rhinoceros* has a Power of stretching it out above Six Inches, to a Point, and doubling it round a Stick, or one's Finger, holding it fast; so that, as to that Action, it is not unlike the *Proboscis* of an *Elephant*.

As to the Tongue of the *Rhinoceros*, although it is confidently reported by Authors, that it is so rough as to be capable of rubbing a Man's Flesh from his Bones; yet that of our present Animal is soft, and as smooth as that of a Calf; which I have often felt, having had my Hand sucked several times by him. Whether it may grow more rough, as the Beast grows older, we cannot say.

His Eyes are dull and sleepy, much like those of a Hog in Shape, and situated nearer the Nose than that of any Quadruped I have ever seen; which he very seldom opens intirely.

His Ears are broad and thin towards the Tops, much like those of a Hog; but have each a narrow round Root with some *Rugæ* about it; and rises, as it were, out of a Sinus surrounded with a *Plica*.

His Neck is very short, being that Part which lies between the back Edge of the Jaw and the *Plica* of the Shoulder; on this Part there are Two distinct Folds, which go quite round it, only the fore one is broken underneath, and has a hollow Flap hanging from it, so deep that it would contain a Man's Fist

shut, the concave Side being forward. From the Middle of the hinder one of these Folds or *Plica*, arises another, which, passing backwards along the Neck, is lost before it reaches that which surrounds the fore Part of the Body.

His Shoulders are very thick and heavy, and have each another Fold downward, that crosses the fore Leg; and, almost meeting that of the fore Part of the Body, just mentioned, they both double under the Belly close behind the fore Leg.

His Body, in general, is very thick, and juts out at the Sides, like that of a Cow with Calf. He has a Hollow in his Back, which is mostly forward, but, backwards, the Line or Ridge rises much higher than that of the Withers; and, forming the *Plica* upon the Loins, falls down suddenly to the Tail, making an uneven Line. His Belly hangs low, being not far from the Ground, as it sinks much in the Middle.

From the foresaid highest Point in his Back, the *Plica* of the Loins runs down on each Side between the last Ribs and the Hip, and is lost before it comes to the Belly; but, above the Place of its being lost, another arises, and runs backward round the hind Legs, a little above the Joint: This I call the *crural Fold*, which turns up behind till it meets another transverse one, which runs from the Side of the Tail forward, and is lost before it reaches within Two Inches of that of the Loins.

The Legs of the *Rhinoceros* are thick and strong; those before, when he stands firm, bend back at the Knee, a great Way from a strait Line, being very round, and somewhat taper downwards. ~~The~~ hinder
Legs

Legs are also very strong, bending backwards at the Joint to a blunt Angle, under which the Limb grows smaller, and then becomes gradually thicker, as it approaches the Foot; so also does that Part of the fore Leg. About the Joint of each of his Legs, there is a remarkable *Plica* when he bends them in lying down, which disappears when he stands.

In some Quadrupeds, the Fetlock bends or yields to the Weight of the Animal; but in this there is no Appearance of any such Bending, and he seems to stand on Stumps, especially if he is viewed behind. He has Three Hoofs on each Foot forwards; but the back Part is a great Mass of Flesh, rough like the rest of his Skin, and bears upon the Sole or Bottom of his Foot.

This Part is plump and callous in the Surface, yielding to Pressure from the Softness of the subjacent Flesh. Its Shape is like that of a Heart, having a blunt Apex before, and running backward in a broad Basis. The Out-line of the Bottoms of the Hoofs are somewhat semicircular.

The Tail of this Animal is very inconsiderable, in proportion to his Bulk, not exceeding 17 or 18 Inches in Length, and not very thick: It has a great Roughness round it, and a kind of Twist or Stricture towards the Extremity, ending in a Fatness, which gave occasion to Authors to compare it to a *Spatula*. On the Sides of this flat Part, a few Hairs appeared, which were black and strong, but short: The Growth of these is seen in the Tail of the old *Rhinoceros*, described very well by Dr. Grew, in his *Museum Regalis Societatis*, which is represented Fig. 2. Plate III. “ In this the Dock is about half an Inch
“ thick,

“ thick, and Two Inches broad; of what Length
 “ the Whole, is uncertain, this being only Part of it,
 “ though it looks as if cut off near the Buttock: It
 “ is about Nine Inches, black, and very rough. On
 “ the Two Edges, and there only, grow also very
 “ black and shining Hairs, a Foot long, stubborn,
 “ and of the Thickness of a smaller Shoe-maker’s
 “ Thread: Yet not round, as other Hair, but rather
 “ flattish, like so many little Pieces of Whalebone.”
 It is further to be observed, that the Hairs on the
 Left Side grow out a great way up towards the Root
 of the Tail, (being shorter, as they are higher) like
 the Fibres of a Quill; whereas, on the Right Side,
 they grow no higher than the flat Part. There is no
 other Hair on any Part of this young *Rhinoceros*,
 except a very small Quantity, on the posterior Edge of
 the upper Parts of the Ears. I have observed a very
 particular Quality in this Creature, of listening to any
 Noise or Rumour in the Street; for though he were
 eating, sleeping, or under the greatest Engagements
 Nature imposes on him, he stops every thing sud-
 denly, and lifts up his Head, with great Attention,
 till the Noise is over.

The *Penis* of the *Rhinoceros* is of an extraordinary
 Shape: It is represented by Fig. 3. Plate III. There is
 first a *Theca*, or *Præputium*, arising from the Inguinal
 Part of the Belly, nearly like that of a Horse, which
 conceals (as that does) the Body and Glans, when
 retracted. As soon as the Animal begins to extend it,
 the first thing that is extruded, the *Theca*, is a second
 Sheath of a light Flesh-colour, and pretty much in
 Form like the Flower of the *Digitalis floribus pur-*
pureis; and then out of this another hollow Tube,
 which

which is analogous to the *Glans Penis* of other Creatures, very like the Flower of the *Aristolochia floribus purpureis*; but of a lighter or fainter Flesh-colour than the former. His Keeper, who was a Native of *Bengal*, would make him thus emit his *Penis* when he pleased, while he lay on the Ground, by rubbing his Back and Sides with Straw; and, in its utmost State of Erection, it never was extended to more than about Eight or Nine Inches. Its Termination is backward in a curved Direction, so that he is a retromingent Animal, and consequently retro-generative. I have several times seen him pissing; he turns his Tail to the Wall, and, extending his hind Legs asunder, crumps himself up, and pisses out in a full Stream as far as a Cow.

We need say no more of the Female *Rhinoceros*, that came over since, but that she is exactly like this in all respects, except the Sex; and, by the Horn, and Size, of the same Age; and the *Pudenda* like those of a Cow.

The Skin of the *Rhinoceros* is thick and impenetrable: In running one's Fingers under one of the Folds, and holding it with the Thumb at the Top, it feels like a Piece of Board half an Inch thick. Dr. *Grew* describes a Piece of one of these Skins tanned, which, he says, "is wonderful hard, and of that Thickness, exceeding that of any other Land Animal he has seen." It is covered all over, more or less, with hard Incrustations like so many Scabs; which are but small on the Ridge of the Neck and Back, but grow larger by degrees downwards toward the Belly, and are largest on the Shoulders and Buttocks, and continue pretty large upon the Legs all
along

along down; but, between the Folds, the Skin is as smooth and soft as Silk, and easily penetrated; of a pale Flesh-colour, which does not appear to View in the Folds, except when the *Rhinoceros* extends them, but is always in View under the fore and hinder Parts of the Belly; but the Middle is incrufted over like the rest of the Skin. To call these scabbed Roughnesses Scales, as some have done, is to raise an Idea in us of something regular; which in many Authors is a great Inaccuracy, and leads the Reader into Errors; for there is nothing formal in any of them.

As to the Performance of this Animal's several Motions, let us consider the great Wisdom of the CREATOR, in the Contrivance that serves him for that Purpose. The Skin is intirely impenetrable and inflexible; if therefore it was continued all over the Creature, as the Skins of other Animals, without any Folds; he could not bend any way, and consequently not perform any necessary Action; but that Suppleness in the Skins of all other Quadrupeds, which renders them flexible in all Parts, is very well compensated in this Animal by those Folds; for, since it was necessary his Skin should be hard for his Defence, it was a noble Contrivance, that the Skin should be so soft and smooth underneath, that, when he bends himself any way, one Part of this Board-like Skin should slip or shove over the other; and that these several Folds should be placed in such Places of his Body, as might facilitate the Performance of every voluntary Motion he might be disposed to.

I only beg leave to add one Paragraph more, wherein I shall attempt to settle a Point that concerns the double Horn mentioned by *Martial*,

Namque

Namque gravem gemino cornū sic extulit ursum *.

And which has given many Critics a great deal of Trouble to alter, as believing either *Martial*, or his Transcribers, were wrong in that Sentence.

There is no where a greater Instance of the Uncertainties that Mankind may be led into from conjectural Reasoning, than in this very Subject of the *Rhinoceros's* Horn. And although the several Critics who have handled this Matter, shew abundance of Ingenuity in changing *Martial's* Reading; yet if we can make it appear, that there was a *Rhinoceros* with Two Horns on his Nose in *Rome*, then that *Poet* was right; if not, *Bochart* has the better, who has altered it thus:

Namque gravi geminum cornū sic extulit urum †.

The first Knowledge we had in this Part of the World of that Animal, was of the one that was brought from *Asia* to the King of *Portugal*, mentioned before; and as those brought into *England* since that Time, *viz.* that in 1685. our present Subject in 1739. and the Female *Rhinoceros* in 1741. were single horned; and as likewise the great Number of Horns that are to be found in the *Museums* of the Curious, brought from time to time from the *East-Indies*, are also single; we may venture to assert, that all those of *Asia* have really but one Horn upon the Nose: And this is confirmed by many Gentlemen, who had seen those Creatures in *Persia*, and other Parts of the East. From thence it is easy to conclude, that this was the Reason the single Horn was imagined

* *Martial*, Epigr. Lib. IV. Epigr. 82.

† *Bochart*, Tom. I. Lib. 3. pag. 931.

the Standard of Nature for that Animal, and that therefore *Martial* ought rather to say, that Two *Bears*, or (according to *Bochart*) Two wild *Bulls*, were tossed by the strong Horn of the *Rhinoceros*; than that a single *Bear* was thrown up by his double Horn.

On the other hand, we are sure, that the *Romans* had always a very great Commerce with the *Africans*, and had many Cargoes of wild Beasts from that Quarter of the World. Is it not therefore likely, that they might more conveniently have obtained the several *Rhinoceros's* that were shewed in that City, from *Africa* than *Asia*; since the Passage to *Italy* from the former is but a short one, cross the *Mediterranean* Sea; and that the Countries that produce those Animals in the latter, are so very remote from *Italy*? For we find the *Greeks* had no Knowledge of this Beast in the Time of *Aristotle*, nor since, that we know of; whereas the *Romans*, according to the Accounts given, have had Six; One shewed by *Pompey the Great*, One by *Augustus*, Two by *Domitian*, One by *Antoninus Pius*, and the last by *Heliogabalus*.

Now we do not want sufficient Proofs to shew, that there is a *Species* of those Animals in *Africa*, having Two Horns on the Nose. *Peter Kolbé*, a *Dutchman*, in his Voyage to the *Cape of Good Hope*, says, there is one on the Summit of the Nose, like the others, but having a smaller clove behind it. There are also Two Horns in Sir *Hans Sloane's Museum*, sticking to the same individual Integuments, not much more than an Inch from each other; which is an undeniable Proof of the Existence of this *Species*; see the Plate III. Fig. 8. And, in
fine,

fine, the Brass Medal of *Domitian*, which you, Sir, were so kind to shew me, has, on one Side, the Figure of a *Rhinoceros* with Two Horns * upon the Nose, very plain. From all which I cannot but be inclined to believe, that this Medal was struck from one of those of *Africa*; and that *Martial* had no more Notion of a *Rhinoceros* with One Horn, than *Bochart* had of one with Two.

There is one thing remarkable of *Albert Durer*. It is certain, from his Print of this Animal, that he, or somebody else, concerned in his Figure, thought that *Martial* was right; for it is plain, they were willing to add a Second Horn to the Figure, and, being puzzled where to place the other, at last put it upon the Neck; by which it further seems probable that *Albert* never saw the Beast †, but was led by the Poet's *Epigram* to make that Addition to the Drawing sent to him from *Portugal*.

Augustini also, in his *Dialogue of Medals*, has a Figure of the *Rhinoceros*, with Two Horns on the Nose. So hath likewise the Figure in the *Prænestan*

* *Pausanias*'s Testimony is of great Force here, having seen then himself in *Rome*, brought thither from *Ethiopia*, with a double Horn on the Nose. His Words are :

Εἶδον ὃ καὶ ταύρους τέρας τε Αἰθιοπικὸς, ὃς ἐπὶ τῷ συμβεβηκότι ὀνομάζουσι ῥινοκέρας, ὅτι σφίσιν ἐπ' ἀκρὰ τῆ ῥινὸς ἐν ἐκείνῳ κέρας, καὶ ἄλλο ὑπὲρ αὐτὸ ἐκείνου, ἐπὶ δὲ τῇ κεφαλῇ ἐστὶ δερχὴν κέρας ἰδίον.

Vidi etiam Tauros Æthiopicos, quos ex re ipsa Rhinocerotas nominant, quod illi e nare extrema cornu prominent; & paulo superius alterum, non sane magnam, in capite nullum prorsus habent. Pausan. Lib. IX. C. 21.

† *Petrus Maffejus* makes this certain: He says, that the *Rhinoceros* that arrived in *Portugal* in 1513. was sent by the King to the Pope, and that the Ship which had him on board was cast away, and the Animal drowned on the Coast of *Genoa*.

Pavement, made by Order of *Sylla* the Dictator, on which he certainly designed to represent several Animals, and other remarkable Things, proper to *Africa*.

Explanation of the Plates of the Rhinoceros.

Plate I.

A side View of the Rhinoceros.

Plate II.

Fig. 1. *A fore View of the Rhinoceros, fore-shortened.*

Fig. 2. *A back View of the same, fore-shortened.*

Plate III.

Fig. 1. *Two Views of one of the Feet. a, the upper Part of the Foot. b, the Sole of the Foot.*

2. *The Tail of an old Rhinoceros, in the Museum of the ROYAL SOCIETY.*

3. *The Penis in an erected State. a, The first Theca or Præputium, of a dark Colour. b, The second Theca, being Flesh-coloured. c, The Tubular Glans Penis.*

4. *A Horn of a Rhinoceros, said to be Six Years old, being about 10 Inches long.*

5. *The Bottom or concave Basis of the same, to shew the Cavity is very superficial.*

6. *A beautiful Horn in Dr. Mead's Museum, being about 37 Inches long.*

7. *The Horn of a Rhinoceros, in the Museum of Sir Hans Sloane, which (as those of Oxen are sometimes liable to Distortions in their Growth) differs from the common Form; it is 32 Inches long.*

VIII. *The*

8. *The double Horn mentioned above, belonging to Sir Hans Sloane: Whether they crossed each other on the Animal, is uncertain: It is most likely they did not, but that by drying they were crossed by the Corrugation of the Skin that joins them together: However, I have drawn them as they appeared to me. The strait Horn is 25 Inches long, the curved one somewhat shorter, and the Two Diameters of the Bases 13 Inches.*
9. *The concave Bottoms of the above double Horns, as they adhere to the same Piece of Skin.*

IX. *An Account of a Comparison lately made by some Gentlemen of the ROYAL SOCIETY, of the Standard of a Yard, and the several Weights lately made for their Use; with the Original Standards of Measures and Weights in the Exchequer, and some others kept for public Use, at Guild-hall, Founders-hall, the Tower, &c.*

Read June
16. 1743.

WHEN there were some time since prepared by Order of the ROYAL SOCIETY, to be kept in their Archives here, and also in those of the *Royal Academy of Sciences at Paris*, Standards of the *Yard Measure*, as also of the *Troy* and *Averdupois Weights*; an Account of which was
some

some Months since published by Order of the COUNCIL of the SOCIETY in these *Transactions* * : It was not at all the Intention of the SOCIETY, to determine what was the absolute legal Length of the *Yard*, or the real and legal Weight of the said several *Pounds* ; but only to lodge and preserve, in those respective Places, Two Measures, and Two Sets of those Weights, sufficiently near to what were in common Use, and well agreeing with each other, for the Purpose of comparing together, by some certain Standard, to which recourse might be had in either Kingdom, the Success of such Experiments made either in *England* or in *France*, in which Measure or Weight might particularly be concerned.

And for the same Reason, the Gentlemen of the *Royal Academy of Sciences*, were pleased to take care to have the Length of their *Half-Toise* set off on both the Brass Rods, upon which the *English Yard* had been already laid off, and to provide Two Brass Weights of *Two French Marcs* each ; one of which was sent over hither, when one of the Brass Rods, just mentioned, was again returned over to the SOCIETY. And it was the Proportion only between *These* several Standards, that was proposed to be laid down in the said Paper published in these *Transactions* ; without intending thereby to ascertain the just and legal Proportions between the Weights and Measures of both Nations. Though it is not to be doubted, but that this Measure of the *French Half-Toise*, and the *French Two Marc Weight*, are, like the *English*, sufficiently agreeable to what are there constantly used.

* N^o 465. p. 185.

But as some Gentlemen have since been desirous to know, how far those Standards really agreed with the Original ones, as they are looked upon to be, in the *Chamberlain's Office* of His MAJESTY's *Exchequer*, as well as with those kept for public Use, at *Guild-hall*, at *Founders-hall*, with the *Watchmakers Company*, and in the *Tower of London*. Mr. *George Graham*, F. R. S. was thereupon requested, with such other Assistance as he should find necessary, to take upon him the Comparison of the said several Standards; which he has accordingly done, and carefully viewed and examined the same, at the *Exchequer*, on *Friday* the 22d of *April* last, in the Presence of the *President* of the SOCIETY, the Right Honourable the Earl of *Macclesfield*, the Right Honourable the Lord *Charles Cavendish*, *John Hadley*, Esq; *William Jones*, Esq; *Peter Daval*, Esq, and *Cromwell Mortimer*, M. D. one of the Secretaries; and at *Guild-hall*, *Founders-hall*, and the *Tower*, on the *Wednesday* following, the 27th of the same Month, in the Presence of all the same Persons, Mr. *Daval* only excepted, who happened to be otherwise engaged that Day. All which Gentlemen were received with the greatest Civility and Regard, by the several Officers who have the Care and Keeping of the respective Standards in Question; who most readily favoured them with the free Use and Inspection of the same; and several of which were themselves also pleased to attend the Examination.


And, as the COUNCIL of the SOCIETY have now thought fit to direct an Account to be here published of these Trials and Experiments: We shall first, for Order-sake, begin with the *Measure* of the *Yard*; and then

then proceed to what concerns the several Weights of the *Troy* and *Averdupois Pounds*.

The Standards of Length now used in the *Enchequer*, are Two squared Rods of Brass, of the Breadth and Thickness of about half an Inch; the one called the *Yard*, and the other the *Ell*. The Ends of neither are exactly flat and parallel, or, if they were so once, they have since suffered some Bruise or Damage, and that possibly by the impressing near each End the Seal of a crowned *E.*; by which it appears, they were placed here during the Reign of Queen *Elizabeth*, and, probably, at the same time when the several Standard-weights, hereafter mentioned, were lodged here also.

To these Rods there belongs a substantial Brass Bar, of about the Length of 49 Inches, the Breadth of an Inch and a half, and the Thickness of an Inch: On one Edge of this Bar is a hollow Bed or Matrix, fitted to receive the square Rod of a *Yard*; and on another, a like Bed fitted to receive that of an *Ell*: And into these Beds they usually fit the *Yard* and *Ell* Measures brought to be examined and sealed at this Office. The square *Yard* and *Ell* Rods fit sufficiently well into these respective Beds, so as neither to rub or shake very sensibly; yet, as neither the Ends of the Rods, or of the hollow Beds, are accurately flat and parallel, the greatest Lengths of those Beds must, of necessity, be somewhat greater than the greatest Lengths of the Rods intended to be placed in them: By which greatest Lengths of those Rods, and which were looked upon by all the Gentlemen present, as the real and proper Lengths of those Rods, are meant the Distances of Two parallel Planes or Cheeks, so placed

placed as to touch the Rods respectively at both Ends.

Besides all which, there also remains in this Office an old Eight-sided Rod of Brass, of the Thickness of about half an *Inch*, very coarsely made, and as rudely divided, into *Three Feet*, and One of those *Feet*, into 12 *Inches*. This is marked near each End with an old *English*  crowned; and is supposed to have been the old Standard of a *Yard*, lodged there in the Time of King *Henry* the Seventh, and used as such, till the other above-mentioned, and now accounted the Standard, was made to supply its Place.

Now, as the *Yard* is from very old time mentioned in our Acts of Parliament, as containing *Three Feet*, or 36 *Inches*; and the *Ell* is not therein particularly described, though universally reputed equal to one *Yard* and a *Quarter*, or to 45 *Inches*; we shall in the following Comparison suppose, that the Length of the square Brass *Yard Rod*, here kept, and marked with a crowned *E*. by that Length meaning, as above, its greatest Length between Two parallel Planes, to be the true and genuine Length of the *English Yard*, or of *Three English Feet*: And with that Length we shall compare the others here mentioned, expressing how much they respectively exceed, or fall short of, this supposed Standard Measure.

To examine all which, Mr. *Graham* was provided with very exact and curious Beam-Compasses of different Sorts, and adapted to the several Purposes they were to be used for. One of these was by parallel Cheeks intended for the taking the Lengths of the Standard Rods above-mentioned to be kept in the *Exchequer*: Another was by rounded Ends, one of

B b b b

which

which was moveable, designed to take the Lengths of such Standards as consist of hollow Beds or Matrices, like those already spoken of at the *Exchequer*, and the others, to be presently mentioned, at *Guild-hall*: And a Third Beam-Compass was fitted in the common way, with fine Points, for the taking off, or laying down, such Measures as are marked out by the Distance of Points or Lines, on any plane flat Surfaces. All which Compasses were severally so contrived, as to be lengthened by the turning of a fine Screw, one of whose Revolutions answered accurately to the 40th Part of an *Inch*, and to which there was applied an Index, shewing, on a small circular Plate with 20 Divisions, the broken Part of a Revolution; and whereon the Place of the Index might, by the Eye, be estimated to about the 10th Part of a Division; whereby the Motion of the moveable Cheek, End, or Point, might consequently be judged of, to about the 8000th Part of an *Inch*.

But Mr. *Graham*, when he determined by these Instruments the following Particulars, desired it might be observed, that although the Alterations of the Compasses were sensible to so small a Quantity; it was not to be supposed the Measures here taken with them, could be estimated to the same Exactness. The Hand cannot judge with so much Nicety, of the Shake of a Rod, when applied between the Cheeks, or when let into one of the hollow Beds or Matrices above-mentioned: Neither can the Eye, though assisted with a Magnifying-glass, pretend to see, with that Accuracy, the Place of the Compass-points, when applied to the taking off a Measure, set out by Points or Lines, on the plane Surface of a Rod or Rule.

Rule. All he therefore thinks possible, and that he has found he could several times together, under the same or like Circumstances, be consistent in, is to take such Measures to about the 1600th Part of an Inch.

We shall, however, in what follows, give those Measures as they actually did come out, in Revolutions, Divisions, and Tenths: All which are also, for the Convenience of the Reader, in a second Column, reduced to the common *Decimals* of an *Inch*; and, in a Third, to the *Vulgar Fractions* of the same.

It may further be noted, that the absolute Quantity of all Measures, any ways inscribed on Standards of Metal, must, from the Nature of Things, vary with the Alterations in the Heat or Coldness of the Weather; and, for that Reason, the exact Proportion between any Two Standards, taken at different times, cannot be expected to be found the same to the most perfect Degree of Exactness, unless the Temperature of the Air shall at those different times have been the same, or that a proper Allowance has been made for the Alteration of it. Yet, in the present Case, as all the several Measures referred to, are inscribed on the same Metal, Brass, as none of the Differences we are concerned about are very great, and as the Change of the Weather was not very considerable between the Days of Trial; it has been thought this last Consideration might safely be neglected, in setting down the following Particulars. Which are, that

The greatest Length of the Matrix of the <i>Yard Measure</i> , at the <i>Exchequer</i> , exceeded the square <i>Standard Yard</i> by - .	$\left. \begin{array}{l} \text{Rev. Div.} \\ 8,2 = .0102 = \frac{1}{97.56} \end{array} \right\}$
--	--

The *Yard* inscribed on
 the *Royal Society's* } Rev. Div
 Rod, exceeded the } 6,0 = .0075 = $\frac{1}{133.3}$
 same by . . . }

The old Brass Standard }
 at the *Exchequer*,
 marked with the } 5,7 = .0071 = $\frac{1}{140.3}$
 crowned D, fell short
 of the same by . . . }

The Standard *Ell Rod*, }
 at the *Exchequer*, ex-
 ceeded 45 *Inches*, of } 1 : 19,5 = .0494 = 20.25.
 such as the Standard
Yard contains 36, by }

At *Guildhall*, the Standards of long Measure there
 used, are only Two Beds, or Matrices, the one of a
Yard, and the other of an *Ell*, cut out of Two of
 the Edges of a substantial Brass Bar, much like that at
 the *Exchequer*, but not altogether so thick; which
 Bar is sealed with the *Exchequer Seal*, and marked
 at both Ends with *C. R.* crowned; and also, as it
 seems, with *W. M.* crowned in like manner. But
 there are here no Rods fitted to these Beds; so that
 all that seemed requisite and proper to be done, was
 to take both the greatest Lengths of these Beds, and
 also the least Lengths of the same; the last being
 nearly the Lengths of such square Rods as might be so
 fitted into the Beds, as to go in every way close, and
 without sensibly shaking: And, upon taking the said
 Measures, it appeared, that

The greatest Length of
the *Yard Bed*, at *Guild-*
hall, exceeded the } Rev. Div.
Standard *Yard*, at the } $1 : 14,7 = .0434 = \frac{1}{23.04}$
Exchequer, by . . }

The least Length of the
same *Bed*, exceeded }
the said Standard of a } $1 : 11,7 = .0396 = \frac{1}{25.2}$
Yard by }

The greatest Length of
the *Ell Bed*, at *Guild-*
hall, exceeded 45 *Ex-*
chequer Standard } $1 : 15,5 = .0444 = \frac{1}{22.5}$
Inches by }

The least Length of the
same *Bed* exceeded }
the same Number of } $1 : 0,7 = .0258 = \frac{1}{38.6}$
like *Inches* by . . }

The Standard of a *Yard*, in the *Tower* of *London*, belongs to his MAJESTY's *Office of Ordnance*, and is kept in the *Drawing Room* there: It is a solid Brass Rod, about Seven-tenths of an *Inch* Square, and about 41 *Inches* long; on one Side of which is laid off the Measure of a *Yard*, divided into *Three Feet*, and each *Foot* into 12 *Inches*: The First *Foot* has the *Inches* divided into Tenths, the Second into Twelfths, and the Third into Eighths of an *Inch*, and the First *Inch* of all is divided into a Hundred Parts, by diagonal Lines. This Rod is said to have been provided by the late Mr. *Rowley*; it is sealed with the *Exchequer* Seal, and Two other Seals of G. R. crowned, near one of the Ends, together with his MAJESTY's Mark commonly called the *Broad Arrow*. And the

the Length of the *Yard*, or of the *Three Feet* inscribed upon it, exceed the forementioned *Exchequer Standard* of a *Yard* by

$$\left. \begin{array}{l} \text{Rev. Div.} \\ 0 : 8,9 = .0111 = \frac{1}{90} \end{array} \right\}$$

The *Standard Yard*, belonging to the *Clockmaker's Company*, was delivered to them from the *Exchequer*, by Indenture, the 4th of *September*, 23 *Car. II. A.D. 1671*. It is a *Brass Rod* of Eight Sides, near half an *Inch* in Thickness, sealed with the *Exchequer Seal*, and *C. R.* crowned, near each End; and whereon the Length of the *Yard* is expressed, by the Distance between Two upright Pins, or small Cheeks, filed away to their due Quantity: This was procured by Mr. *Graham*, to be brought to the *President's House* of the *ROYAL SOCIETY*, on *Saturday* the 7th of *May* last, where all the above-named Company then met, to collate their respective Notes of these several Trials, all which were found to agree with each other: At which last Meeting, Mr. *John Machin*, of *Gresham College*, the other Secretary of the *SOCIETY*, was present also: And the Length of this last *Yard Measure* was then tried, and found to fall short of the *Exchequer Standard Yard Measure*, now very carefully added

$$\left. \begin{array}{l} \text{on the Middle Line of} \\ \text{the ROYAL SOCIETY's} \\ \text{Brass Rod,} \end{array} \right\} \begin{array}{l} \text{Rev. Div} \\ 0 : 16,8 = .021 = \frac{1}{47.62} \end{array}$$

Now, as to the *Weights*, those in the *Chamberlain's Office* in his *MAJESTY's Exchequer*, and which are esteemed the *Standards*, are a *Pile*, or *Box*, of hollow *Brass Troy Weights*, from *CCLVI Ounces* downwards, to the 16th Part of one *Ounce*, all severally marked with a crowned *E.*: But they have no *Penny-*

Penny-weights, or *Grain Weights*, that are any ways esteemed or looked upon as Standards.

The Weight mentioned in all our old Acts of Parliament, from the Time of King *Edward* the First, is universally allowed to be the *Troy Weight*, whose *Pound* consisted of Twelve *Ounces*, each of which contained Twenty *Penny-weights*: And as the Pound is the Weight of the largest single Denomination commonly mentioned in those Acts, Twelve *Ounces* taken from the Pile of *Troy Weights* above-mentioned, as there is no single *Troy Pound Weight*, must now be reputed the true Standard of the *Troy Pound*, used at this Day in *England*.

Besides which *Troy Standards*, there are also kept in the *Exchequer* the following Standards for *Averdupois Weights*: That is to say, a *Fourteen Pound Bell Weight* of Brass, marked with a crowned *E.* and inscribed

XIII. POVNDE AVERDEPOIZ.
ELIZABETH. REGINA.

1582.

as also a *Seven Pound*, a *Four Pound*, a *Two Pound*, and a single *Pound*, like *Averdupois Bell-Weights*, and severally marked as follows, excepting the Variations for the Number of *Pounds* in each respective Weight.

VII. A.

AN^o  D^o

E. L.

1588.

A^o REG. XXX.

With which are also kept a Pile of flat *Averdupois Weights*, from 14 *Pounds* down to the 64th Part of a *Pound*.

When

When the *Averdupois* Weight came first to be looked upon as a lawful Weight, does not appear; but by these Standards it is plain, it has been used as such, ever since the Reign of Queen *Elizabeth*. And as the Weight of 15 *Pounds Averdupois*, has before been made use of, in determining the Proportion between the Weight of this *Pound* and that of the *Pound Troy*, we shall begin by giving the Counterpoise of the said 15 *Pound Averdupois*, as it was found in *Troy* Weight: From whence we shall deduce the Proportions of those *Pounds*, and afterwards compare the same with the like Proportions, deduced from the *Seven Pounds*, and single *Pound* Bell-Weights, and the single *Pound* flat Weight above-mentioned: All which Weights were taken in the Presence of the above-named Noblemen and Gentlemen, by Mr. *Samuel Read*, Scale and Weight-maker near *Aldersgate*, who brought to the *Exchequer* a large Balance of his own for that Purpose, and which, when loaded with 15 *Pounds* at each End, was very readily turned with Six *Grains*; as a lesser one he brought also for examining the single *Pound* Weights, was with half a *Grain*. He also brought with him what he called his own Standard *Penny* and *Grain* Weights, to supply what was necessary to make the Counterpoise of the *Exchequer* Weights: With all which the Result was, that

The Standard 14 *Pound*, and single *Pound Averdupois* Weights, taken together, were, upon a Medium of Four Trials, after counterchanging the Weights in each *Basin*, changing the *Basins*, and then again counterchanging the Weights, found to be counterpoised by 218 *Troy Ounces*, 13 *Penny-weight*,

weight, 23 *Grains* and One-fourth. From whence the *Averdupois Pound* is deduced equal to 6998.35 of such *Grains* as the *Troy Ounce* is reputed to contain 480 of; and the *Averdupois Ounce*, of which 16 are supposed to make a *Pound*, is found equal to 437.4 like *Grains*.

Again: The *Seven Pound Bell Averdupois Weight*, with the same Scales, and upon a Medium of Four like Experiments, counterchanging, as before, both Weights and Basons, was found to be counterpoised by 102 *Troy Ounces One Penny-weight*, and 21 *Grains*. According to which, the *Averdupois Pound* comes out equal to 7000.7, and the *Ounce* to 437.54 *Troy Grains*.

Again: The single Bell *Averdupois Pound*, with the lesser Scales, on the Medium of Two Experiments, counterchanging the Weights, the Basons not being moveable, was found to weigh 14 *Troy Ounces 11 Penny-weight* and 18 *Grains*; or was equal in Weight to 7002, and the *Ounce* to 437.62 *Troy Grains*.

The single *Averdupois Bell Pound*, against the flat *Averdupois Pound Weight*, was found, on a Medium of Two like Experiments, to be heavier by Two *Troy Grains* and a half: Whence the flat *Averdupois single Pound Weight* weighs only 6999.5, and the *Ounce* 437.46 *Troy Grains*.

The ROYAL SOCIETY'S *Averdupois Pound* was, in like manner, found to be lighter than the *Exchequer single Bell Pound Weight*, by One *Grain*.

And their *Troy Pound Weight* to be lighter than the Eight and Four *Ounce Troy Weights* at the *Exchequer*, taken together, by half a *Grain*.

The *Founders Company* of *London* are, by their Charter from King *James* the First, *authorized and directed to have the sizing and marking of all manner of Brass Weights, to be made or wrought, or to be uttered, or kept for Sale, within the City of London, or Three Miles from the same.* And the *Weights* delivered to them from his MAJESTY'S *Exchequer*, and now kept in their Hall, as their Standards for the Uses above-mentioned; are a *Pile* of flat *Brass Troy Weights*, from *CCLVI Ounces*, down to the 16th Part of an *Ounce*, all sealed with the *Exchequer Seal*, and marked with *C. R.* crowned 1684. and a Stamp of the initial Letters of the Maker's Name: As also a Set of Bell *Brass Averdupois Weights*, sealed and marked in like manner. And here the following Trials were made, before the above-named Gentlemen, by Mr. *Read*, but with a large Balance, commonly used for Trials at the Hall, in their Office for that Purpose; and found to turn with about the same Weight as the former; and also with a lesser one, turning in like manner under these Circumstances, with about half a *Grain*, which Balance belonged likewise to the Hall, as did also the *Penny* and *Grain Weights* made use of, but which were not kept by them as Standard Weights.

And here it was found, that, on a Medium of Four Trials, made in like manner as before, at the *Exchequer*, that 15 *Pounds Averdupois*, being their 14 *Pounds*, and single *Pound Standard Weights*, were counterpoised by 218 *Troy Ounces*, 15 *Penny-weight* and 23 *Grains*: Whence the *Averdupois Pound* is deduced equal to 7001.53, and the *Ounce* to 437.59 *Troy Grains*.

Again:

Again: The single *Averdupois* Standard Pound weighed, on a Medium of Two Experiments, countering the Weights, as before, 14 *Troy Ounces*, 11 *Penny-weight*, $16\frac{1}{2}$ *Grains*: Or was equal to 7000.5, and the *Ounce* to 437.53 *Troy Grains*.

Again: This Standard *Averdupois* Pound, at a Medium as before, outweighed the ROYAL SOCIETY'S *Averdupois* Pound, by Two *Grains* and One-eighth: And the *Troy* Standards of Eight and Four *Ounces*, taken together, outweighed the ROYAL SOCIETY'S single *Troy Pound* Weight, by Two *Grains* and One-eighth, at a like Medium.

At his MAJESTY'S *Mint* in the *Tower* of *London*, their Standard Weights are only a Pile of *Troy* hollow Weights, from CCLVI *Ounces*, down to the 16th Part of One *Ounce*, without any *Penny* or *Grain* Weights. The larger of these Weights, as low as the VIII *Ounce-weight*, are marked with *A. R.* crowned, and inscribed PRIMO MAIL, A° DNI. 1707. A° REGNI VI°. The III and the II *Ounce* Weights are only marked with *A. R.* crowned, without the Date; and the lesser have only the *Exchequer* Seal, and the *Rose* and *Crown*, being the Mark of his MAJESTY'S *Mint*, as all the larger ones have also. And here it was found by Mr. *Joseph Harris*, one of the *Assay-Masters* of the *Mint*, with a very curious Balance of his own, fixed in a Glass Lantern, and which he was well assured might in such Circumstances be depended upon to less than half a *Grain*; and with the Addition of so many *Penny* and *Grain* Weights belonging to his Office as were necessary: that

The ROYAL SOCIETY's whole *Troy Pound* Weight weighed, at a Medium, less than the Eight *Ounces* and Four *Ounces* of these Standards, taken together, by Two *Grains* and Three-eighths.

That the ROYAL SOCIETY's *Averdupois Pound* weighed in *Troy* Weight by these Standards, 14 *Ounces* 11 *Penny Weight* 16 *Grains* and Seven-eighths; or 7000.87 *Grains*.

That the ROYAL SOCIETY's Pile of 16 *Ounces Troy*, was lighter than 16 *Ounces* of these Standard Weights, by Four *Grains* and Three-fourths.

And lastly, That the ROYAL SOCIETY's Eight *Ounces* and Four *Ounces* together, taken from their Pile, weighed lighter than their single *Troy Pound* Weight, by Five-eighths of a *Grain*.

X. *The Description of an Instrument for reducing a dislocated Shoulder; invented by Mr. John Freke, Surgeon of St. Bartholomew's Hospital, and F. R. S.*

GENTLEMEN,

Read June
23. 1743.

I Should not have presented this to you, but to shew in how small a Compass the whole Power which can be made use of in reducing a dislocated Shoulder can be contracted. If therefore a Machine for this Purpose be not portable, it matters but little to an afflicted Patient Ten Miles off, how good an Instrument is out of his Reach.

This

This Machine (see TAB. IV. Fig. 2.) which consists of Two Boxes *A*, joined at the Ends by Two Hinges, contains, when folded together, every thing that can possibly be wanted in the Operation before-mentioned; and it may so easily be made use of, without the Assistance of any other Operator than the Surgeon employed, that I may venture to affirm, a Patient may be set down, the Instrument applied, and the Shoulder reduced, in One Minute, ordinarily speaking.

The Length of this Instrument, when shut up, is One Foot Eight Inches, its Breadth Nine Inches, and Thickness Three Inches and a Quarter. When it is opened, it is kept so by Two Hooks fixed on the Backside of it; and when one End of it stands on the Ground, the other stands high enough to become a *Fulcrum*, or Support of a Lever *BB*, which is fixed on a Roller *b*, by a large Wood Screw, which turning sideways as well as with the Rower, it obtains a circumrotatory Motion, so that it will serve to reduce a Luxation either backward, forward, or downward.

The Roller on which the Lever is fixed, is just the Diameter of the Depth of one of the Boxes, into which are driven Two Iron Pins, the Ends of which are received by the Two Sides of the Box, which are an Inch thick.

The Lever is Two Foot Four Inches, and is cut off and joined again by Two Hinges *C*, to fold up so as to be contained in the Boxes. On the Backside of it is a Hook, to keep it strait; the other End of it is to hang over the Roller about an Inch and half, which is to be excavated and covered with Buff Leather, for the

the more easy Reception of the Head of the *Os humeri*.

Two Iron Cheeks *DD* are screwed on each Side of the Lever, to receive through them an Iron Roller *E*, which has Two Holes through it, to receive Two Cords coming from a Brace *F*, fixed on the lower Head of the *Os humeri*; for on no other Part of the Arm above the Cubit can a Bandage for this Purpose be useful; for, if the Surgeon applies it on the muscular Part of the Arm, it never fails slipping down to the Joint, before you can extend the Limb.

The Iron Roller has a square End, on which is fixed a Wheel *G*, within the Cheek, notched round, which works as a Rotchet on a Spring Ketch underneath the Lever, by which it is stopped, as you wind it with a Winch; and may at Pleasure be let loose, as there shall be Occasion for it, by discharging the Ketch.

I come now to describe the Brace *F*, which, compared with common Bandages, is of more Consequence than can easily be imagined by unexperienced Persons. It consists of a large Piece of Buff Leather, big enough to embrace the Arm, sewed on Two Pieces of strong Iron curved Plates, riveted together, one of them having an Eye at each End, to fasten Two Cords in; the other is bent at the Ends into Two Hooks, which are to receive the Cords, after they have crossed over the Arm above.

In order to keep the Patient steady in his Chair from coming forward, or letting the *Scapula* rise up, on depressing the Lever, after the Limb is drawn forward by the Winch, there must be fixed over the Shoulder a Girth with Two Hooks at the Ends of it,
long

long enough to reach to the Ground on the other Side, where it must be hooked into a Ring *I*, to be screwed into the Floor, for that Purpose.

XI. *A Letter from Pierce Dodd, M.D. Fellow of the Royal College of Physicians, London, and Physician to St. Bartholomew's Hospital, to the President of the ROYAL SOCIETY, concerning a Person who made bloody Urine in the Small-pox, and recovered.*

S I R,

Read June 23.
1743.

MAKING bloody Water is universally esteemed as terrible a Symptom as any that can happen in the *Small-pox*; and all who have wrote concerning that Distemper, do unanimously agree, that it is a certain Forerunner of approaching Death. Dr. *Cade*, indeed, says, in his Letters to Dr. *Freind*, concerning Purging in that Distemper, that he has sometimes cured this Symptom, by the Help of *Camphire*, and a copious Quantity of Acids; but then he adds, that this Relief was only temporary; and that, to confess the Truth, he never knew any body, that made that sort of Urine, who ever survived the 16th Day from the Eruption: And there is nobody whom I know, that has been conversant with this Distemper, but has constantly experienced, sooner or later, the like Fatality in consequence of it. I mean, when this sort of Urine has
pro

proceeded from a broken *Crafs* and Contexture, or, as it were, a thorough Diffolution of the whole Mass of Blood: For I know very well, that you shall now-and-then have several Streaks, and sometimes larger Quantities of Blood in the Urine, from the Acrimony of the *Spanish Flies*, upon the Application of Blisters, which are frequently used, and so frequently likewise absolutely necessary, in one or other of the Stages of this Distemper, and yet the Patient shall do well. And Dr. *Browne*, who was a worthy Predecessor of mine in *St. Bartholomew's Hospital*, and a Gentleman of Learning and Eminence in his Profession, gives an Account of a Gentlewoman, who lived in *Dean's-yard, Westminster*, who made bloody Urine in the *Small-pox*, Four or Five Days together; which made Dr. *Needham*, who attended her, to forsake her; and yet she recovered: But they found afterwards, that this bloody Water was not occasioned by the Malignancy of the Distemper, but by a sharp Stone, which was at that time descending from one of the Kidneys through the Ureters into the Bladder, and which she afterwards voided.—— This I have from some *Adversaria* of his, a good Number of which I have had the Fortune to be in Possession of several Years, and amongst which there are several remarkable Cases, that occurred to him during the Course of a long and extensive Practice; and which I do not know, but may, some time or other, be communicated to the Public, together with some others that have happened to fall within the Compass of my own Observation, since I have had the Honour to have something to do in the Faculty, in that Hospital, and elsewhere.

But

But to return to our Subject:

It cannot but be a Satisfaction to a Person of your Humanity and Goodness therefore, to hear of an Instance in which this frightful Symptom has not been attended with its usual Fatality: It was in the Case of a young Spark, about 15 Years of Age, Son to a Gentleman of a very considerable Fortune in *Jamaica*.

He was taken with a Fever, and great Pain in his Head, *April* 20th last, and had the *Small-pox* come out upon him the Day following, notwithstanding which the same Symptoms still continued, and nothing almost would stay upon his Stomach, and his Head likewise was very delirious: He was obliged therefore to be bled, and to take a Vomit, and to have Blisters applied to his Neck and to his Arms; which, together with a proper Quantity of *Pulvis e Chelis Cancrorum comp.* and *Nitre*, were the first things, that I had an Opportunity of ordering for him.

The next Day every thing was more quiet, and so again the Third Day from the Eruption; but the *Small-pox* were very numerous all over him, and of a little, rank, angry Sort; as they generally are, I think, upon the *West-India* Constitutions: But this young Gentleman had besides over-heated himself a little before, by performing a Part at the *Mountain*, near *Eton*, where he was a Scholar.

Things continued in much the same State the 4th Day, but towards the Evening there were a few Streaks of Blood mixed with, and subsiding in his Water; which did not much alarm me, because I did not know but it might be caused by the Blisters. I had but one Reason to doubt the contrary, and that was, he had had little or no Strangury: But as cer-

tain Persons do aver, there is sometimes such, or even a more bloody sort of Waters, occasioned by the Flies, even where there shall be no Strangury at all, I was willing to hope the best, and so made no other Alteration in my Process, than to direct a very free Use of *Spirit of Vitriol*.—

What was ordered, happened to succeed: We had no more of that Sort of Water, either that Night, or the next Day, or the Morning following: But I was sent for in a great Hurry that Day, *viz.* the 6th, in the Afternoon, and found his Friends in the most terrible Consternation; not only because it returned, but began to increase upon them, and was pouring off in a free Manner.—

It was necessary therefore to proceed in another Method, and I accordingly ordered some *Gum Arabic*, *Olibanum*, and *Pulvis Amyli*, and *Alum*, together with a Mixture of *Black Cherry-water* and small *Cinnamon*, and *Treacle-water*, with some *Tinctura Antiphthistica* and *Terra Japonica* in it, and the *Tincture of Roses*, strongly acidulated and sweetened with *Diacodium*; upon the Use of which it began to abate, and the next Day the Urine returned to its usual State and Colour.—

There was nothing farther observable in the Course of this Case, except that the Distemper was of the Coherent kind, and accordingly attended with many other dubious Symptoms likewise: For though it is generally thought, that the Coherent sort is not so formidable as the Confluent; yet, as Dr. *Freind* has judiciously observed, and *Moreton* before him, there is not so much Difference between them, but they are almost always attended with much the same Appearances, and the same

same Fevers plainly at the time of Maturation : For that the Danger does not arise so much from the Sort, as from the Number of the Pustules; which if it be great, there is the like Reason to be fearful of the Event, whether they flux, or whether they only cohere: All which notwithstanding, this young Gentleman had the good Fortune to escape.

I have nothing farther to detain you with at present, but one short Request; and that is, that you will be so good as to believe me to be, with all the Respect and Esteem imaginable,

S I R,

June 23. 1743.

Your most Obedient,

Faithful Servant,

Peirce Dod.

June 23. 1743. the SOCIETY adjourned to Oct. 27.

L O N D O N :

Printed for T. WOODWARD, and C. DAVIS, over-
against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS
to the ROYAL SOCIETY, M.DCC.XLIV.

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Fig. 1.

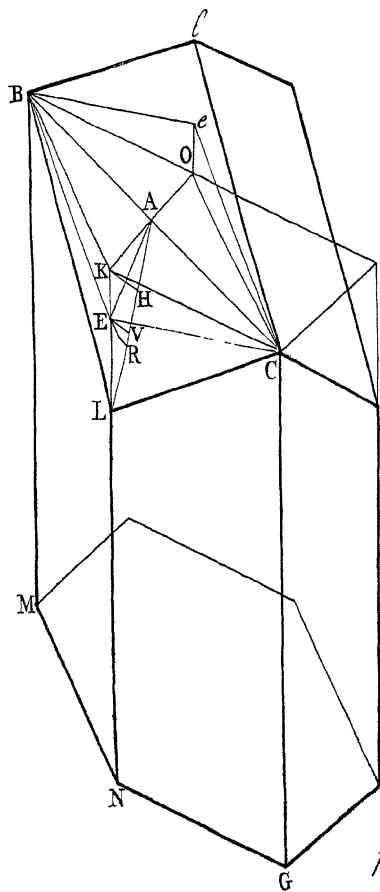


Fig. 2.

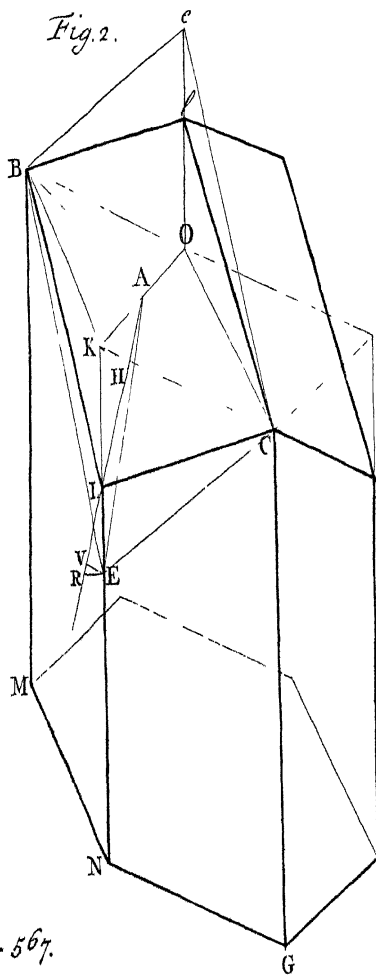
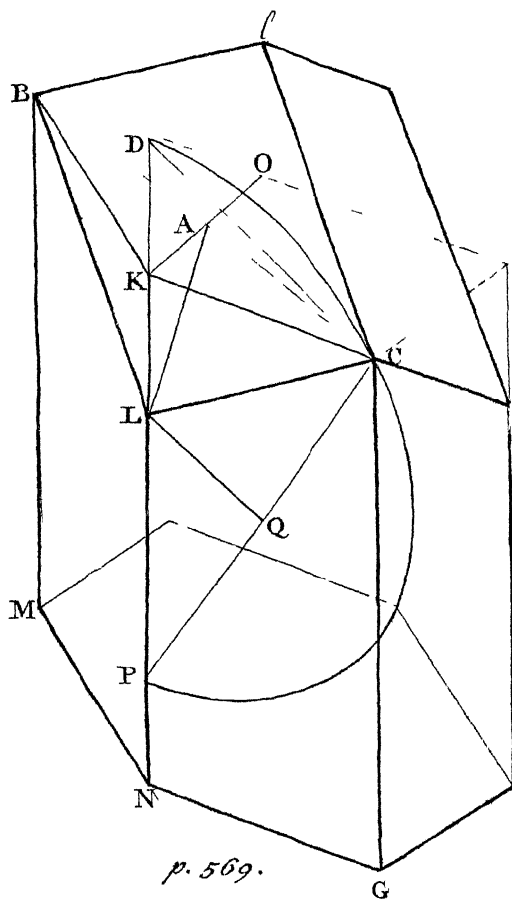
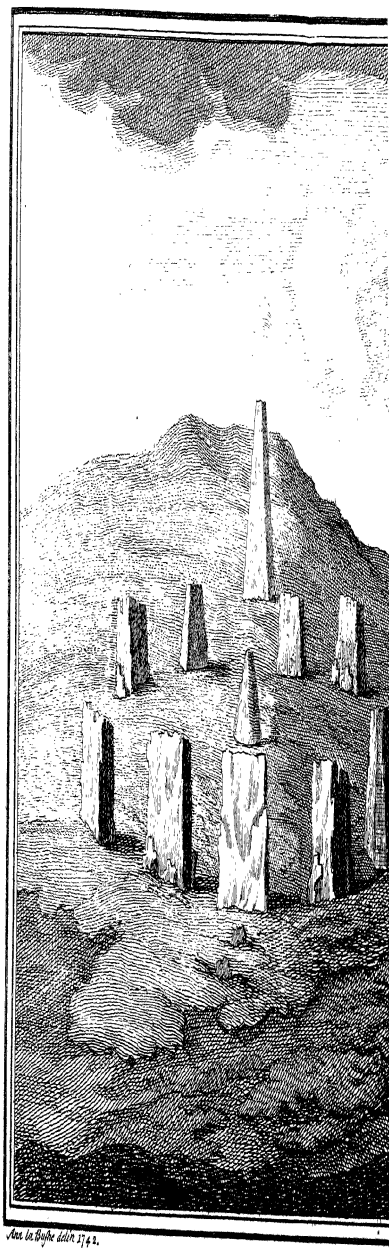
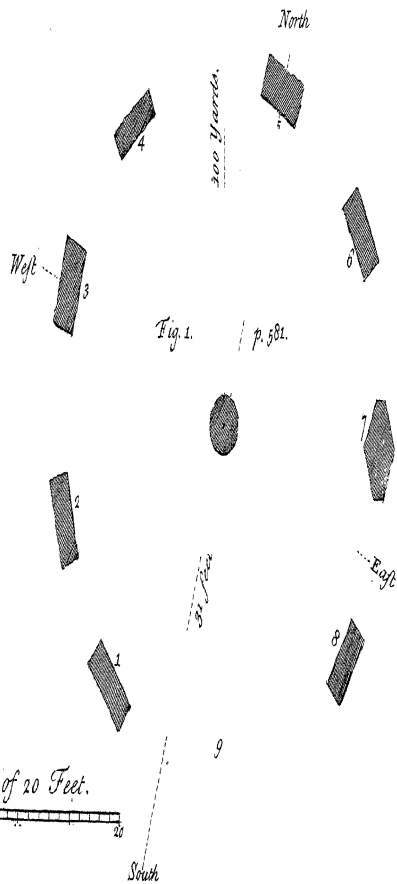
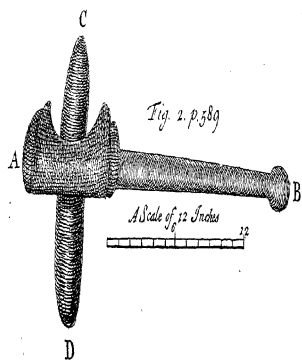


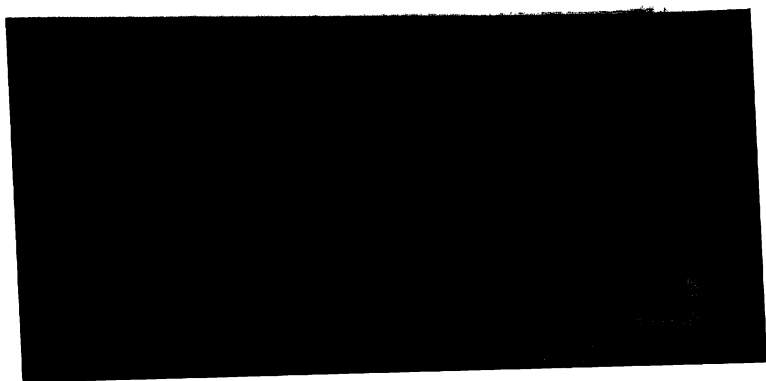
Fig. 3.







J. Mynde sc.



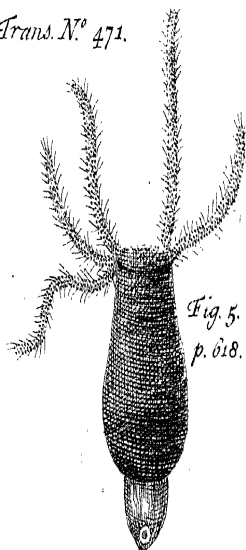


Fig. 5.
p. 618.

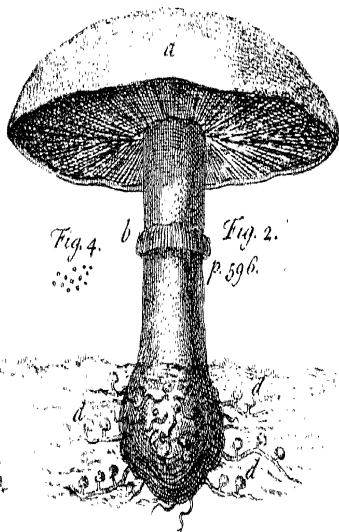


Fig. 2.
p. 596.



Fig. 3.
a.

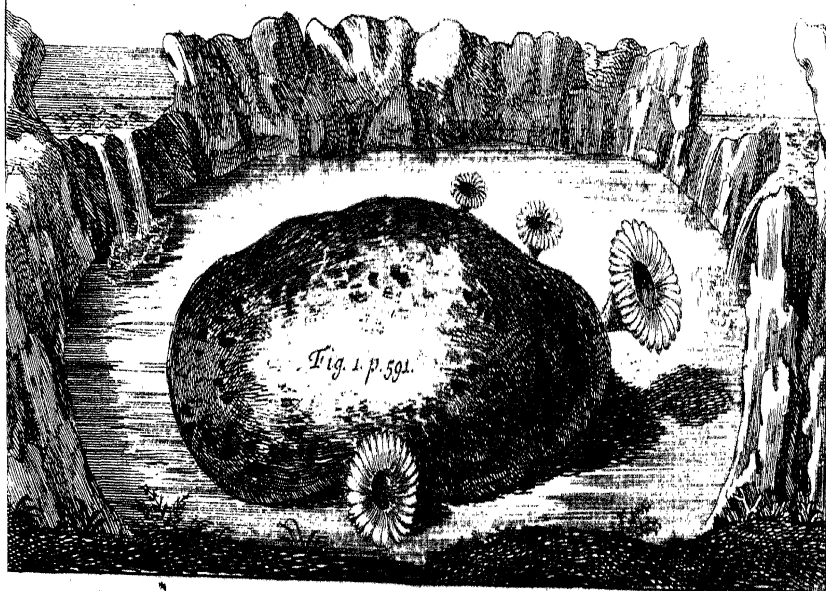


Fig. 1. p. 591.

PHILOSOPHICAL TRANSACTIONS

For the Months of *November* and *December*, 1741.

The CONTENTS.

- I. *Of the Bases of the Cells wherein the Bees deposit their Honey. Part of a Letter from Mr. Mac Laun, Professor of Mathematics at Edinburgh, and F. R. S. to Martin Folkes, Esq; Pr. R. S.* Page 565.
- II. *A Letter from Mr. John Winthrop, Hollisian Professor of Mathematics and Astronomy at Cambridge in New-England, to C. Mortimer, M. D. Sec. R. S. concerning the Transit of Mercury over the Sun, April 21. 1740. and of an Eclipse of the Moon, Dec. 21. 1740.* 572.
- III. *An Account of the Transit of Mercury over the Sun, Oct. 25. 1743. in the Morning, observed at Mr. Geo. Graham's House in Fleetstreet.* 578.
- IV. *An Eclipse of the Moon, Oct. 22. 1743. in the Morning, observed at Mr. Graham's House in Fleetstreet.* 580.
- V. *Part of a Letter from the Right Reverend Father in God ROBERT Lord Bishop of Corke, and F. R. S. to the Right Honourable JOHN Earl of EGDMONT, F. R. S. concerning the Remains of an antient Temple in Ireland, of the same sort as the famous Stonehenge, and of a Stone Hatchet of the antient Irish.* 581.
- VI. *A Letter from the Reverend Mr. Griffith Hughes, Minister of St. Lucy's Parish in Barbadoes, to Martin Folkes, Esq; Pr. R. S. concerning a Zephyton, somewhat resembling the Flower of the Marigold.* 590.
- VII. *A Letter to the Rev. Mr. Roger Pickering, F. D. M. to Cromwell Mortimer, M. D. Sec. R. S. concerning the Seeds of Mushrooms.* 593.

The CONTENTS.

- VIII. *Some Remarks occasioned by the preceding Paper addressed to the ROYAL SOCIETY by Mr. William War-
son, Apothecary, and F. R. S.* 599.
- IX. *De Dispositione Annali Saturni An. 1743 &c. 1744.
in Epistola a D^{no} Golofredo Heinsio ad D^{um} Petrum
Colluculentum, R. S. S. data.* 602.
- X. *Attestation of a Natural History of Greenland, by
Hans Egedius, intitled, Det gamle Gronlands Per-
illustration, eller Naturel-Historie, af Hans Egede.
Kiøbenhavn, 1741. 4^{to}. Communicated by John Green,
M. D. Secretary of the Gentlemens Society at Spalding.* 607.
- XI. *Some Observations on a Polype dried. By Mr. Henry
Baker, F. R. S.* 616.
- XII. *A Catalogue of the FIFTY PLANTS from Chelsea
Garden, presented to the ROYAL SOCIETY by the Company
of Apothecaries, for the Year 1740. pursuant to the Di-
rection of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg.
Præf. By Joseph Miller, Apothecary, Hort. Chel. Præf.
& Præf. Botan.* 620.
- XIII. *Epistola Johannis Bevis, M. D. ad Gul. Jones,
Armig. R. S. S. de Transitibus Mercurii sub Sole, Oct.
31. 1736. & Oct. 25. 1743.* 622.
- XIV. *Part of a Letter from M. Geoffroy, F. R. S. and
Member of the Royal Academy of Sciences at Paris, to Sir
Hans Sloane, Bart. late President of the ROYAL SO-
CIETY, concerning a Child of a monstrous Size.* 627.
- XV. *Two remarkable Medical Cases, one of an Extraordi-
nary Hæmorrhage, the other an Ascites cured by Tapping;
by Henry Banyer, M. D. Extra-Licentiate of the College
of Physicians, London, to C. Mortimer, M. D. Secr. R. S.* 628.
- XVI. *A Letter from Mr. Turbervill Needham, to the PRI-
SIDENT; concerning certain chalky tubulous Concretions,
called Malm: With some Microscopical Observations on the
Farina of the Red Lilly, and of Worms discovered in
Smutty Corn.* 634.
- I. Of

I. *Of the Bases of the Cells wherein the Bees
deposite their Honey Part of a Letter
from Mr. Mac Laurin, Professor of Ma-
thematics at Edinburgh, and F. R. S. to
Martin Folkes, Esq; Pr. R. S.*

June 30. 1743.

*Presented to
the Royal So-
ciety Nov. 3.
1743.*

THE Sagacity of the *Bees* in making their Cells of an hexagonal Form, has been admired of old; and that Figure has been taken notice of, as the best they could have pitched upon for their Purposes: But a yet more surprising Instance of the Geometry of these little *Insects* is seen in the Form of the Bases of those Cells, discovered in the late accurate Observations of Monsieur *Maraldi* and Monsieur *de Reaumur*, who have found those Bases to be of that Pyramidal Figure, that requires the least Wax for containing the same Quantity of Honey, and which has at the same time a very remarkable Regularity and Beauty, connected of Necessity with its Frugality.

These Bases are formed from Three equal *Rhombus's*, the obtuse Angles of which are found to be the Doubles of an Angle that often offers itself to Mathematicians in Questions relating to *Maxima* and *Minima*; that is, the Angle, whose Tangent is to the *Radius*, as the Diagonal is to the Side of the Square. By this Construction, of the Six solid Angles at the Base that correspond to the Angles of the

E e e e

Hexa-

Hexagon, Three are equal as well to each other, as to the solid Angle at the *Apex* of the Figure, each of which solid Angles is respectively formed from Three equal plane obtuse Angles: And the other Three solid Angles are also equal to each other, but severally formed each from Four equal plane acute Angles, Supplements to the former obtuse ones.

By this Form the utmost Improvement is made of their Wax, of which they are on all Occasions very saving, the greatest Regularity is obtained in the Construction, and with a particular Facility in the Execution; as there is one sort of Angle only with its Supplement, that is required in the Structure of the whole Figure.

Monsieur *Maraldi* * had found by Mensuration, that the obtuse Angles of the *Rhombus's* were of 110 Degrees nearly; upon which he observed, that if the Three obtuse Angles which formed the solid Angles above-mentioned, were supposed equal to each other, they must each be of $109^{\circ}.28'$; from whence it has been inferred, that this last was really the true and just Measure of them: And lately Monsieur *de Reaumur* † has informed us, that Mr. *Koëning* having, at his Desire, sought what should be the Quantity to be given to this Angle, in order to employ the least Wax possible in a Cell of the same Capacity, that Gentleman had found, by a higher *Geometry* than was known to the Antients, by the Method of *Infinitesimals*, that the Angle in question ought in this Case to be of $109^{\circ}.26'$. And we shall now make

* *Memoires de l'Acad. Royale des Sciences*, 1712.

† *Memoires sur les Insectes*, Tom. V.

it appear from the Principles of common *Geometry*, that the most advantageous Angle for these *Rhombus's* is indeed, on that Account also, the same which results from the supposed Equality of the Three plane Angles that form the above-mentioned solid ones.

Let GN and NM represent any Two adjoining Sides of the Hexagon, that is, TAB. I.
Fig 1. and 2. the Section of the Cell perpendicular to its Length. The Sides of the Cell are not complete Parallelograms as $CGNK$, $BMNK$, but *Trapezia* $CGNE$, $BMNE$, to which a *Rhombus* $CEBe$, is fitted at E , and that has the opposite Point e in the *Apex* of the Figure, so that Three *Rhombus's* of this kind, with Six *Trapezia*, may complete the Figure of the Cell. Let O be the Centre of the Hexagon, of which CK and KB are adjoining Sides; join CB and KO , intersecting it in A ; and, because COB is equal to CKB , and KE equal to Oe , the Solid $EBCK$ is equal to the Solid $eBCO$; from which it is obvious, that the Solid Content of the Cell will be the same, where-ever the Point E is taken in the Right Line KN , the Points C, K, B, G, N , and M , being given. We are therefore to inquire where the Point E is to be taken in KN , so that the *Area* of the *Rhombus* $CEBe$, together with that of the Two *Trapezia* $CGNE$, $ENMB$, may form the Least Superficies. Because Ee is perpendicular to BC in A , the *Area* of the *Rhombus* is $AE \times BC$, that of the *Trapezia* $CGNE$, $ENMB$, is $\overline{CG + EN} \times KC$; these, added to the *Rhombus*, amount to $AE \times BC + 2KN \times KC - KE \times KC$; and because $2KN \times KC$ is invariable, we are to

inquire, when $AE \times BC - KE \times KC$ is a *Minimum*?

Suppose the Point L to be so taken upon KN , that KL may be to AL as KC is to BC . From the Centre A describe in the Plane AKE with the Radius AE , an Arc of a Circle ER meeting AL , produced, if necessary, in R ; let EV be perpendicular to AR in V , and KH be perpendicular to the same in H ; then the Triangles LEV , LKH , LAK , being similar, we have $LV:LE::LH:LK::LK:LA::$ (by the Supposition last made) $KC:BC$. Hence, when E is between L and N , we have $LH+LV(=VH):LK+LE(=KE)::KC:BC$; and when E is between K and L , we have $LH-LV(=VH):LK-LE(=KE)::KC:BC$; that is, in both Cases we have $KE \times KC = VH \times BC$; and consequently $AE \times BC - KE \times KC = AE \times BC - VH \times BC = \overline{AE - VH} \times BC = \overline{AR - VH} \times BC = \overline{AH + VR} \times BC$; which, because AH and BC do not vary, is evidently Least when VR vanishes, that is, when E is upon L . Therefore $CLBL$ is the *Rhombus* of the most advantageous Form in respect of Frugality, when KL is to AL as KC is to BC . This is the same Method by which we have elsewhere determined the *Maxima* and *Minima*, in the Resolution of several Problems that have usually been treated in a more abstruse Manner. See *Treatise of Fluxions*, Art. 572, &c.

Now because OK is bisected in A , $KC^2 = OK^2 = 4AK^2$; and $AC^2 = 3AK^2$, or $BC = 2AC = 2\sqrt{3} \times AK$; consequently $KC:BC::2AK:2\sqrt{3} \times AK::1:\sqrt{3}$, and $KL:AL::(KC:BC)::1:\sqrt{3}$,

$1:\sqrt{3}$, or $AL:AK::\sqrt{3}:\sqrt{2}$; and (because $AK:AC::1:\sqrt{3}$) $AL:AC::1:\sqrt{2}$; that is, the Angle CLA is that, whose Tangent is to the Radius as $\sqrt{2}$ is to 1, or as 14142135 to 10000000; and therefore is of $54^{\circ}. 44'. 08''$, and consequently the Angle of the *Rhombus* of the Best Form is that of $109^{\circ}. 28'. 16''$.

By this Solution it is further easy to estimate what their Savings may amount to upon this Article, in consequence of this Construction. Had they made the Base flat, and not of the pyramidal Form described above, then, besides completing the Parallelograms $CGNK$ and $BMNK$, the Surface of the Base had been $3CB \times AK$; what they really do form amounts in Surface to the same Parallelograms, and $3CB \times AH$: the Savings therefore amount to $3CB \times AK - AH = 3CB \times AH \times \frac{\sqrt{1}-\sqrt{2}}{\sqrt{2}}$, which is almost a Fourth-part of the Pains and Expence of Wax, they bestow above what was necessary for completing the parallelogram Sides of the Cells: And at the same time they seem also to have other Advantages from this Form, besides the saving of their Wax; such as a greater Strength of the Work, and more Convenience for moving in these larger solid Angles.

It remains that we should shew, that the plane Angles CLB , CLN , and BLN , are equal to each other. We before found, that $KL:AL::KC:BC::KA:(=\frac{1}{2}KC)AC$; consequently $KL:KA::AL:AC$, and the Triangles LKA , LAC , are similar: Therefore $LK:AL::AL:LC::KC:BC::1:\sqrt{3}$, and $LC=3LK$. With the Centre L and Radius LC , describe in the Plane $CGNK$ the Semicircle DCT , meeting the Line KN , in D and P ;

TAB. I.
Fig. 3.

join

join CP and CD , and let LQ be perpendicular to CP in Q , then will the Angle CDK be equal to QLP , and we shall have $PQ : LQ :: PC : DC :: \sqrt{PK} : \sqrt{DK} :: \sqrt{LC+LK} : \sqrt{LC-LK} :: \sqrt{4} : \sqrt{2} :: \sqrt{2} : 1 :: AC : AL$. Consequently the Angle $QLP = ALC$, and $CLP = CLB$, or the obtuse Angle of the *Rhombus* $CLBl$ is equal to CLP , the obtuse Angle of the *Trapezium*; and consequently, the Three plane Angles that form the solid Angle at L , or the *Apex* at l , are equal to each other: From which it is obvious, that the Four acute plane Angles, which form the solid Angle at C or B , are likewise equal among themselves.

Though Monsieur *Maraldi* had found, by his Mensuration, these obtuse Angles to be of about 110 Degrees; the small Difference between this and the $109^{\circ}. 28'. 16''$, just found by Calculation, seems to have been either accidental, or owing to the Difficulty of measuring such Angles with Exactness: Besides that he seems to admit the real Equality of the several plane Angles, that form as well the *Apex*, as the other solid ones we have been treating of. And, as to the small Difference between our Angle and that determined by Mr. *Koëning*, who first considered this Problem, but has not yet published his Demonstration of it, that can only be owing to his not carrying on his Computation so far, and would scarcely have been worth the mentioning, were it not yet in Favour of the Practice of these industrious little Insects; and did it not therefore give us ground to conclude, that in general, and when the particular Form and Circumstance of the Honey-comb does not require a Variation from their Rule,

the

the *Bees* do truly construct their Cells of the best Figure, and that not only nearly, but with Exactness; and that their Proceeding could not have been more perfect from the greatest Knowledge in *Geometry*. How they arrive at this, and how the wonderful Instinct in Animals is to be accounted for, is a Question of an higher Nature; but this is surely a remarkable Example of this Instinct, as it has suggested a Problem that had been overlooked by Mathematicians, though they have treated largely on the *Maxima* and *Minima*; and such an one, as has been thought to exceed the Compass of the common *Geometry*.

It may be worth while to add here, that if the Cells had been of any other Form than hexagonal, and the Bases had still been pyramidal, these must have been terminated by *Trapezia*, and not by *Rhombus's*, and therefore had been less regular, because *OA* and *AK* would have been unequal: Nor could there have been room for such an advantageous or frugal a Construction as that we have described, because the solid Content of the Cell would have increased with the Right Line *KE*. The Cells, by being hexagonal, are the most capacious, in proportion to their Surface, of any regular Figures that leave no Interstices between them, and at the same time admit of the most perfect Bases. Thus, by following what is best in one respect, unforeseen Advantages are often obtained; and what is most beautiful and regular, is also found to be most useful and excellent.

II. *A Letter from Mr. John Winthrop, Hollisian Professor of Mathematics and Astronomy at Cambridge in New-England, to C. Mortimer, M. D. Sec. R. S. concerning the Transit of Mercury over the Sun, April 21. 1740. and of an Eclipse of the Moon, Dec. 21. 1740.*

S I R,

Read Nov. 3.
1743.

THOUGH I have not the Honour to be known to you, I flatter myself you will excuse the Freedom of this Letter, since the Design of it is to lay before you an Observation which, I hope, may be of some Use in Astronomy. In Confidence of this, I take the Liberty to inform you, that, on the 21st of *April* 1740. I had an Opportunity to observe *Mercury*, then near his descending Node, transiting the Sun's Disk. Being advertised by the Calculations of that excellent Astronomer Dr. *Halley*, that the former Part of this Transit would be visible in our Horizon, I was resolved to observe it in the best manner I could, with those few Instruments I was furnished with; which were only those I had received from my Predecessor Mr. *Isaac Greenwood*, and are the same that are mentioned by the late Mr. *Thomas Robie* in *Philosophical Transactions* N^o 382. being a 24 Foot Telescope, another of Eight Foot, and a brass Quadrant of Two Foot *Radius*, fitted with telescopic Sights, and having cross Hairs fixed in the *Focus* of the Glasses. All these I got in
Readi-

Readiness, being the more desirous to make this Observation, because *Mercury* had never as yet been seen entering upon or going off the Sun's Limb at his descending Node, and this *Transit* ought to be invisible to *Europe*. The better to observe *Mercury's* Ingress on the Sun, I determined to make use of my 24 Foot Tube, while an Assistant I had with me used that of Eight Foot : After which I proposed, in order to find out *his* Path in the Sun, to observe the Passages of *Mercury* and the Sun's Limbs by an horizontal and vertical Hair in the Telescope of the Quadrant ; and I chose rather to deduce *Mercury's* Right Ascensions and Declinations by Calculation from hence, than to observe them immediately in the common way of placing one of the cross Hairs parallel to the Equator, &c. because, as the Sun was likely to be low before *Mercury* made his Entrance, Refraction would have caused considerable Errors in the Places of *Mercury* determined in this Manner. Having no Clock, I was obliged to make use of my Pocket-Watch, which I know to be a good one ; and by this it was easy to distinguish Time to a Quarter of a Minute, which would have served pretty well for the Ingress of the Planet. But as it was by no means sufficient for those other Observations I designed to make, I procured another Watch, which shewed Seconds ; and both these Watches I adjusted to the apparent Time, by several Altitudes of the Sun taken with the Quadrant before the *Transit* began ; and by Altitudes taken the next Day, I found that the Watches had kept time exactly enough. I expected that the Centre of the Planet would enter upon the Sun at 5^h 2' ; but, being apprehensive that he might

be earlier than the Calculation, I, for some time before that, with my 24 Foot Tube directed to the Sun, kept my Eye fixed on that Part of his Limb where the Planet was to enter, as steadily as I could for the Wind, which then blew fresh. This Precaution was not needless; for, at $4^h, 54', 59''$, I perceived that *Mercury* had made an Impression on the Sun's Limb; by the Quantity of which I concluded, that almost One quarter of his Diameter might be entered. After I had beheld this very plainly about a Minute, a small Cloud covered the Sun near $3'$; which then clearing off, and the Sun shining very bright, as before, I had again a distinct View of the Planet, and saw much more than half his Body on the Sun. I continued to see him till $5^h 0' 40''$, at which time he seemed to be gotten almost wholly within the Sun; for he appeared now very near round, though I could not yet discern the Sun's Light behind him. By the shaking of the Tube, I unfortunately missed the Moment of his interior Contact with the Sun's Limb, but am certain it could be but very little later than this; for I presently after saw him fairly within the Sun. Upon which, I repaired to my Quadrant; but this being at my Lodgings, at some Distance from the long Telescope with which I observed the Ingress, and which I had no Convenience for raising nearer Home, almost half an Hour slipped away before it was possible for me to begin my Observations. I began them as soon as I could, and continued them till Sun-set, excepting when I was interrupted by the Clouds; and I observed sometimes one and sometimes the other Limb of the Sun, as I found it most convenient. It will be needless, I

sup-

suppose, to give a Detail of all the Observations I made ; I shall therefore select Two or Three, which I look upon as most exact, and most suitable to my present Purpose. One was as follows :

	h.	'	''
The Sun at the Horizontal	5.	37.	59.
The Sun at the Vertical . . .		39.	1.
<i>Mercury</i> at the Vertical . . .		39.	16.
<i>Mercury</i> at the Horizontal . .		40.	1.

This Observation gave me the *Azimuth* and Altitude of *Mercury* at his Passage by the vertical Hair ; from whence I computed his Right Ascension and Declination, and from thence his Longitude and Latitude. The Method of obtaining which being sufficiently known, I shall say nothing upon it, but only mention the Result of the Numbers, which was, that at 5^h, 39', 16'', when *Mercury* passed the Vertical, his Longitude was 12°, 43', 5'', 8 ; and the Sun being then in 12°, 42', 27'' of that Sign, *Mercury* was, in consequence of the Sun's Centre, 38'', his Latitude at the same time being 15', 2'' North. Another Observation was thus :

	h.	'	''
The Sun at the Horizontal . .	6.	47.	37.
The Sun at the Vertical . . .		48.	17.
<i>Mercury</i> at the Vertical . . .		48.	25.
<i>Mercury</i> at the Horizontal . .		49.	24.

From hence I concluded, that at 6^h, 48', 25'', *Mercury* was in Antecedence of the Sun 3', 57'', with 14', 20'' North Latitude. I made another Observation after this ; but the Sun being then very near the Horizon, his Limbs were not well defined, so that I look upon this Observation as much preferable to that. I

shall set down only Two more, which were made about the Middle between these Two; and were made by the Sun's upper Limb.

	h.	'.	''.
The Sun at the Vertical . .	6.	6.	56.
<i>Mercury</i> at the Vertical . .		7.	8.
<i>Mercury</i> at the Horizontal . .		8.	42.
<u>The Sun at the Horizontal . .</u>		9.	45.
	h.	'.	''.

The Sun at the Vertical . .	6.	17.	18.
<i>Mercury</i> at the Vertical . .		17.	29.
<i>Mercury</i> at the Horizontal . .		18.	26.
The Sun at the Horizontal . .		19.	32.

At the former of these Observations; *viz.* 6^h, 7', 8'', I computed the Longitude of *Mercury* to be in 12°, 42' 17'' 8, which being taken from the Sun's Place in 12°, 43', 35'' 8, leaves 1', 18'' for the Difference of Longitude between the Sun and *Mercury*; and his Latitude was then 14', 47''. At the latter Observation, the Difference of Longitude was 1', 55'', and the Latitude of *Mercury* 14', 42''.

From these Places of *Mercury* it appears, that his horary Motion in Longitude from the Sun was now 3', 58''; according to which, if we suppose the central Ingress to have been at 4^h, 57', we shall find the Difference of Longitude at that time 3', 20''; and the Semidiameter of the Sun being 15', 57'', the Latitude of *Mercury* must be 15', 36''. Now the Angle of *Mercury's* visible Way with the Ecliptic being, by the Theory of his Motion, 10°, 23', we must conclude the former of the observed Latitudes about 4'' too small, and the latter as much too large;—an Error very inconsiderable in this kind of Observations.

tions. From these things we may gather by an obvious Computation, that *Mercury* was in Conjunction with the Sun, in respect of Longitude, at $5^h, 47'$, with $14', 59''$, North Latitude; and that his nearest Distance to the Centre of the Sun was $14', 44''$; and when he was at his nearest Distance, the Difference of his Longitude from the Sun's was $2', 39''$, which he passed over in $40'$ of Time, and consequently arrived at the Middle of his Course in the Sun at $6^h, 27'$: Whence the Semiduration of the central *Transit* was $1^h, 30'$, and the End at $7^h, 57'$, an Hour after Sun-set.

As to the Place of *Mercury's* Nodes, the Inclination of his Orbit to the Ecliptic, and the other Elements of his Theory, I pretend not to determine any thing from so short a Series of Observations as this. I content myself with the foregoing Determinations, which, I hope, are not far from the Truth, having taken all the Care I could, both in the Observations and Calculations.

I was in Hopes to have made a good Observation of the Lunar Eclipse, which happened last Week: But the Sky, which at the Beginning of the Eclipse was very clear, soon became overcast, which hindered me from making above One or Two Observations that I could depend upon; and they were as follows:

21 December 1740.

h. '.

- At 5. 24. A plain Penumbra.
- 35. The true Shadow seems to enter.
- 47. Touches *Palus Maræotis*.
- 53. Reaches *Mount Sinai*.

After

After this the Clouds thickened, and covered the Moon till the End of the Eclipse, which was about 8^h, 30', as near as I could guess through the Clouds.

The Night before the Eclipse, *viz.* 20 *December*, at 12^h, 14', I saw the Moon eclipse a Fixed Star, which, I think, is in the Heel of *Castor*.

These Two last Observations were made with an Eight Foot Telescope, my Watch being rectified to the apparent Time by correspondent Altitudes of the Sun, taken with the before-mentioned Quadrant for several Days together, before and after the Eclipse.

I must ask your Pardon for this long Trouble, and am,

S I R,

Cambridge in New-
England, Dec. 30.
1740.

Your most humble Servant,

John Winthrop.

III. *An Account of the Transit of Mercury over the Sun, Oct. 25. 1743. in the Morning, observed at Mr. Geo. Graham's House in Fleetstreet.*

Read Nov. 3.
1743.

THE Beginning could not be seen by reason of Clouds, but about 8^h, 45', *Mercury* was seen (through a reflecting Telescope Three Foot *Focus*, magnifying about 50 times) about Four or Five of his Diameters within the Sun's Limb.

At Mr. *Short's* House in *Surrey-street*, *Mercury* was seen just past the interior Contact 8^h, 30', 59", through

through a reflecting Telescope Two Foot *Focus*, magnifying about 70 times; the Person who observed it says, that the Thread of Light between *Mercury* and the Sun's Limb was so small, as scarcely to amount to the 20th or 30th Part of *Mercury's* Diameter.

The following Differences of Right Ascension between the Sun's preceding Limb and Mercury, were taken at Mr. Short's House.

Sun's preceding Limb touched the	}	h.	l.	".
Wire at		10.	58.	55.
<i>Mercury</i> touched the same Wire at		10.	59.	40.

Sun's preceding Limb touched the	}			
Wire at		11.	48.	4.
<i>Mercury</i> touched the same Wire at .		11.	48.	32.
Sun's subsequent Limb touched the	}			
same Wire at		11.	50.	20.

Sun's preceding Limb touched the	}			
Wire at		11.	49.	20.
<i>Mercury</i> touched the same Wire at		11.	49.	46.

Sun's preceding Limb touched the	}			
Wire at		11.	51.	9.
<i>Mercury</i> touched the same Wire at		11.	51.	36.

Sun's preceding Limb touched the	}			
Wire at		12.	1.	33.
<i>Mercury</i> touched the same Wire at		12.	1.	57.

Mr. *Graham* got an Observation made by a Person in his Neighbourhood, by which it appears, that
at

at 11^h, 59', 50", *Mercury* preceded the Sun's Centre 42" in Right Ascension.

The Sky clearing up towards One o'Clock, the following Times were observed at Mr. *Graham's* House with great Accuracy.

	h.	'	"
Last interior Contact at	1.	0.	42.
End, or <i>Mercury</i> just leaving the	}	1.	2.
Sun's Limb at			
			16.

This last Observation agrees to a Second with the same Observation made by Dr. *Bevis* at Mr. *Sisson's* House in the *Strand*.

During the time of these Observations it blew a violent Gale of Wind, so that both Observers and Instruments were somewhat disturbed.

IV. *Eclipse of the Moon, Oct. 22. 1743. in the Morning, observed at Mr. Graham's House in Fleetstreet.*

Read Nov. 3.
1743.

THE Sky was mostly overcast with Clouds, so that the following Observations are the only ones that could be made with any Degree of Certainty.

	h.	'	"
Beginning of the Eclipse about	1.	21.	0.
The Shade touched <i>Copernicus</i> about	1.	39.	0.
touched <i>Plato</i> about	1.	45.	0.
touched <i>Tycho</i> about	1.	51.	0.
Total Immersion about	2.	17.	0.

V. *Part of a Letter from the Right Reverend Father in God ROBERT Lord Bishop of Corke, and F.R.S. to the Right Honourable JOHN Earl of EGMONT, F.R.S. concerning the Remains of an antient Temple in Ireland, of the same sort as the famous Stonehenge, and of a Stone-Harcher of the antient Irish.*

Read Nov. 10.
1743.

— **A**S I never met with any satisfactory Account of these *antient Temples*, I imagined that a short Account of this one, which I met with in the County of *Corke*, in the Parish of *Kilgarriffe*, when I was upon a parochial Visitation, would not be unentertaining to *Your Lordship*; it stands about 10 Miles from *Bandon* to the South-west.

As to the Drawing TAB. II. Fig. 1. the Ground-plan is exact, but the Upright was not taken upon the Place, but drawn from my Description of it, given to Miss *Bushe*, a young Lady who is related to my Wife, and was then in the House with me. It contains the Representation of a very antient Heathen Temple, and the Burial-place of some Person of great Renown, before the erecting of covered Temples was made use of, in this Part of the World, or perhaps in any other Part of the World, except *Judea*. Which sort of Places of Devotion seem to be the most antient of any that we have Accounts of in History. For *Temples* were originally all open, and thence received their Name, according to *Varro* (*Lib. 6. de ling. Lat.*)

a Templando, which was an antient Word that signified to *see* or *look out*. These Places therefore were called Temples by the Heathens, because they were holy Places, that were marked out by the Augurs for taking their Auguries in; and were therefore left open, that the Priest or Augur, who stood with his Face to the South, according to *Rosinus*, (*Ros. Ant. Lib. III. c. 9.*) or with his Face to the East, according to *Calepine*, (*Cal. Dict. Templum*) might be able to see all around him; his Art of Prediction depending on the Flight of Birds, or some Appearances in the Face of the Sky, which varied their Signification according as they shewed themselves, either on the Right Hand or Left Hand of the Augur. Whether the Disposition of these Stones, in the Plan I have sent you, was designed or accidental, with regard to the Points of the Compass, I cannot say; but it is remarkable, that there are Two Stones, which are fixed directly in the *North* and *West* Points of this Circle; and Two Openings answering to the *East* and *South*: So that it is possible both our Authors may be in the right, and that the Priest sometimes stood with his Face to the East, and sometimes to the South. The middle Stone, which was the Place where the Priest stood, is lower than the rest, not being above Three Feet high, and was always dedicated to some Deity or other; and was consecrated to that Use by the pouring on of Olive-oil: Which Custom was of very antient Date, and seems to have been borrowed from the Practice of the antient Patriarchs, who called these Stones *Bethels*. For when *Jacob* was going from *Beersheba* towards *Padan-Aram* or *Haran*, to seek for a Wife, by Command from his Father

Isaac

Isaac, having laid down to sleep, GOD appeared to him in a Dream; and, when he rose up in the Morning, it is said, that *he took the Stone which he had put for his Pillow, and set it up for a Pillar, and poured Oil upon the Top of it, and he called the Name of that Place BETHEL* (Gen. xxviii. 18. 22.); which Word literally signifies in *English*, GOD's House. Again, when *Jacob* and *Laban* made a Covenant together, *Jacob took a Stone, and set it up for a Pillar*, and probably poured Oil thereon, by way of dedicating it to GOD, as he had done before; for that *Moses* made use of Oil in the Dedication of the Tabernacle, and Altar, and Vessels, &c. is plain from *Lev.* viii. 10, &c. And in this Place, when *Jacob* and *Laban* had finished their Covenant, it is observed, that *Jacob offered up a Sacrifice* (Gen. xxxi. 45. 54.). Again; when *Jacob* afterwards fled from the *Shechemites*, GOD appeared unto him; and in the Place where GOD talked with him, it is said, that *he set up a Pillar, even a Pillar of Stone, and poured a Drink-offering thereon, and he poured Oil thereon. And Jacob called the Name of the Place where GOD spake with him, BETHEL* (Gen. xxxv. 9. 15.). And hence these Stones, which were erected as Marks of these Places having been dedicated to GOD, came to be called *Bethels*; and, by a corrupt Pronunciation of the Word, they were in *Greek* called Βαιβλία (*vide Sanchoniatho*). Which is the Reason why that Stone, which *Rhea* is supposed to have given *Saturn* to swallow instead of a Child, is called Βαιβλος; and not because it was covered with a woollen Garment, which is called Βαιτη in *Greek*, as *Hesychius* pretends. *Hesych. Etym.*

And that this Custom of dedicating single Stones to GOD was not confined to *Judæa*, is plain from *Clemens Alexandrinus*, who observes, that before the Art of Carving was invented, the Antients erected unwrought Pillars, and paid their Worship to them as to the Statues of the Gods (*Clem. Alex. Strom. Lib. I.*). *Herodian* also mentions a Pillar, or large Stone, of a black Colour, and a conical Form, at *Antioch* in *Phœnicia*, which was erected in Honour of the Sun (*Herod. Lib. V.*). *Pausanias* also mentions several of these uncarved Pillars in *Bœotia* in *Greece*, and says they were the antient Statues erected to their Gods (*Pausan. in Bœot. & in Acha.*). And that this Custom continued till after the Time of the Prophet *Isaiah*, is plain from his making use of the Expression of erecting a Pillar to GOD, to denote the Worship of God. For, says he, *In that Day shall there be an Altar to the Lord in the midst of the Land of Egypt, and a Pillar at the Border thereof to the Lord. And it shall be for a Sign, and for a Witness, unto the Lord of Hosts in the Land of Egypt* (*Isa. xix. 19, 20.*). And *Arnobius*, who flourished about 330 Years after *Christ*, says, that this Custom continued to his Time; and that, when he was a Heathen, he never met a Stone, which had the Marks of Olive-oil being poured upon it, that he did not look upon as something divine, and offered up his Prayers to it as such (*Arnob. cont. Gent. Lib. I.*).

As to the Custom of erecting this *Bethel* with a certain Number of Stones around it, this also is to be found in the Old Testament. For it is said of *Moses*, after he had been in the Mount with GOD,
and

and had returned to the People of *Israel*, that *he rose up early in the Morning, and builded an Altar under the Hill, and Twelve Pillars according to the Twelve Tribes of Israel* (Ex. xxiv. 4.): Which Altar was probably surrounded with these Twelve Pillars, or Twelve large Stones, pitched on an End, and stuck in the Ground; for so the Word מצבה, *Matsebah*, literally signifies; as a proper Designation of the Quantity of Ground, which ought to be looked upon as sanctified by the Altar, and dedicated to God. Of the same kind also we may suppose those Twelve Stones to be, which *Joshua pitched in Gilgal*, after the Children of *Israel* had passed the River *Jordan* (Josh. iv. 20.). The Number of Stones which surrounded these *Bethels*, I suppose therefore were intirely voluntary, at the Discretion of the Persons who dedicated the *Bethel*; and might be fewer or more, either according to the Number of Persons principally concerned in the Dedication, or the Size of the Place, or the Number of Stones which could conveniently be found large enough for that Purpose. The Number of those, of which I have sent you the Draught, are Nine, which surround the *Bethel* at 15 Feet and half Distance from the Centre; the Height of each Stone is about Six Feet above-ground, and their Breadth is from Three Feet and half to near Four Feet, some a little more, and some a little less.

The Stone marked (*b*), which stands detached from the rest, I take to be a *Matsebah*, or Pillar erected as a Memorial of the Burial-place of some eminent Person; either the Prince or Priest of the Country, or probably both: For antiently the principal Person of each Family, Tribe, or Nation, officiated both as
 Prince

Prince and Priest: And in *Hebrew* the same Word *Cohen* signifies both Prince and Priest. And what confirms this Opinion of its being a sepulchral Monument is, that some of the ancient Popish Families hereabouts make use of it as a Burial-place to this Day. The first Account we have of this Custom of erecting Stone Pillars on or near the Burying-place of eminent Persons, is that of *Rachel's*, who dying in Child-birth of her Son *Benjamin*, in the Road between *Bethel* and *Ephrah*, it is said, that *Jacob* set a Pillar upon her Grave (Gen. xxxv. 20.). Of the same kind also may we suppose that *Matsebah* or Pillar to be, which *Absalom* erected for himself during his Life-time, though better wrought, and more ornamental, in the King's Dale; where it is more than probable he designed to have been buried; for it is observed, that he said, *I have no Son to keep my Name in Remembrance, and he called the Pillar after his own Name* (2 Sam. xviii. 18.). Which Custom, of erecting Pillars over the Burial-places of eminent Persons, was not confined to the Land of *Judæa*; but was universally practised, as appears from a Passage in *Homer*, where *Minerva* exciting *Telemachus* to go in Quest of *Ulysses*, and supposing the worst that could happen, that is, that he should come to a certain Knowledge of the Death of his Father, she directs him then to raise a Pillar, or Signal, to his Memory; or, as Mr. *Pope* has translated it,

*To the pale Shade Funereal Rites ordain:
Plant the fair Column on the vacant Grave:
A Hero's Honours let the Hero have.*

Hom. Odyf. Lib. I.

And

And hence, in my Opinion, came the Origin of *Obelisks* in *Egypt*, which abounding with the finest Quarries in the World, gave them an Opportunity of pitching Stones of the largest Size over the Burial-places of their eminent Men. And you may observe, that this Stone, of which you have the Plan marked (*b*), is somewhat in the Form of an *Obelisk*; being Ten Feet high, and Two Feet square at the Bottom, diminishing gradually to a Point at the Top.

It is remarkable, that some of these Stones manifestly appear to have been reduced to the Form they are in by Art; particularly that one last-mentioned, as well as the one marked *N. 7.* which is reduced into an hexagonal Form, the inward and the outward Front being similar, with an Angle in the Middle, as represented in the Ground-plan. There is no Appearance of any Mark of a Tool, so that it is probable, that this was done with great Labour, by the Assistance only of sharp Stones; which, before the Invention of Iron, or of that Metal's being common, seems to have been the usual Instrument of Operation in other Circumstances as well as this. For it is observed of *Zipporah*, the Wife of *Moses*, when she was ordered to circumcise her Son, that *she took a sharp Stone, and cut off the Foreskin of her Son* (*Ex. iv. 25.*) And, when *God* orders *Joshua* to circumcise the *Israelites*, he says, *make thee sharp Knives*, as we translate it; but, in the Original it is, *Knives of sharp Stones* (*Josh. v. 2, 3.*).

Herodotus and *Diodorus Siculus* both take notice, that it was the Custom among the antient *Egyptians*, at the time of embalming the Dead, to cut open the Body with an *Ethiopic* Stone (*Herod. Euterp. Diod. Lib. I. c. 5.*): And *Ovid*, in describing the
Origin:

Origin of the Customs of the *Corybantes*, &c. says, that a *Phrygian* Youth with whom the Goddess *Cybele* was in Love, and to whom he proved faithless, for a Punishment * to himself, cut himself all over with a sharp Stone; *Ille etiam saxo corpus laniavit acuto*, &c. (*Ovid. Fast.* 4.).

It is manifest, indeed, that the Use of Iron was found out in *Egypt* before the Time of *Joshua* and *Moses*, both of whom mention it as made use of not only for cutting of soft things †, but also for chizeling of Stones, (*Deut.* xxvii. 5. *Josh.* viii. 31.) But I apprehend it must have been very rare, and that the Art of reducing of Iron to the Hardness and Consistency of Steel, was not yet discovered; because, when God orders *Joshua* to write the Words of the Law upon Stones, as soon as he had passed over *Jordan*, the way he is ordered to do it is this; to plaster the Stones over with Plaster first, and then to grave in this Plaster the Words of the Law (*Deut.* xxvii. 2, 3.). And yet this is called both by *Moses* and *Joshua*, writing upon the Stones (*Deut.* xxvii. 8.).

It is certain, that the Art of polishing of Jewels, and of cutting one hard Stone with another that was harder, was invented and practised in *Egypt* before the Time of *Moses*; for he speaks of graving the Names of the Children of *Israel* in Two Onyx stones, which, being harder than Iron, even than Steel, are not to be wrought upon therewith; but must be cut by some Stone which is harder than themselves.

* Of the Antiquity of this Practice, see *Lev.* xix. 28.

† *Joseph*, when he was sent for by *Pharaoh*, shaved himself, *Gen.* xli. 14.

Wherefore *Moses* says, *with the Work of an Engraver in Stone, like the Engravings of a Signet, shalt thou grave the two Stones* (Ex. xxviii. 9. 11.). And therefore the Prophet *Jeremiah* mentions a *Pen of Iron*, as made use of for Engraving (*Jer.* xvii. 1.).

But the Use of Iron does by no means seem to have been found out in these Western Parts of the World till much later; and therefore it is probable, that the Inhabitants of these Countries made use of Stones, which were the original Instruments used in cutting both for domestic and military Service, in all Countries of the known World; as appears of late Years from the Practice of the *Americans*. And it is also manifest, from the many Instruments of War, that are made of Stone, which have been dug up in these Western Parts of *Europe*, that the Use of Iron was not very common in these Parts, till of late Years. *Montfaucon*, in the IVth and Vth Tome of his *Antiquities*, gives us an Account of several Tombs being opened near *Paris*, and in other Places; wherein the hard and destructive Part of the Weapons found therein consisted of Stone. He particularly gives us the Cut of a Stone Hatchet in his own Possession, which was made of Touchstone, in the 4th Tome of his Supplement, p. 30. But as I have at present in my Possession a much more complete one, made of the same kind of Stone, I have sent you the Draught of it done with Exactness, by a Scale of a quarter of an Inch to an Inch, TAB. II. Fig. 2. and you will see, that it is plainly made for doing Execution both ways; and therefore answers the Description given by *Montfaucon* of the *Amazonian Hatchet*, or the *Sagaris* of *Xenophon* (*vide Montf.* Tom. IV. p. 69.). The Handle

is made of Yew, and the Stone is not inserted into the Handle at Right Angles, but makes an acute Angle below towards the Hand; the Use of which appears at first Sight. I am,

My LORD,

Your most obedient,

Humble Servant,

ROBERT CORKE.

VI. *A Letter from the Revd Mr. Griffith Hughes, Minister of St. Lucy's Parish in Barbadoes, to Martin Folkes, Esq; Pr. R. S. concerning a Zoophyton, somewhat resembling the Flower of the Marigold.*

S I R,

Read Nov.
10. 1743.

HOWEVER surprising the Description of a Flower, which I believe is in Reality an Animal, may appear; yet we cannot, without the highest Arrogance, presume to prescribe Limits to the Power of the ALMIGHTY, who, for wise Ends, sometimes hides his Works in such Darkness, as to be concealed from the most exalted human Knowledge. There are no Ages past, in which fresh and almost numberless Instances of his wonderful Works have not discovered themselves. And what, in ours, seems most inexplicable, will, possibly, appear to Futurity, no more than the natural

tural Consequence of other Discoveries then become familiar.

The following Description of an Animal, which, after Three Years repeated Observation, I always found unvaried in its Appearance, had perhaps been still the Subject of my own silent Admiration, if I had not been lately persuaded by a worthy Member of your Honourable SOCIETY to lay it before you, as I now beg Leave to do.

At the North End of the Island of *Barbadoes*, in *St. Lucy's* Parish, is a Cave about 14 Feet long, and 11 wide: Its Bottom is a Bason always full of transparent Salt-water, its greatest Depth about Three Feet: In this Bason there is a Stone of about Four Feet long, and Three in Breadth, always covered with Water. From small Holes in the Sides of this Stone, at different Depths under Water, appear in full Bloom, at all times of the Year, several seemingly fine radiated yellow Flowers, with thick-set distinct *Petala* *: These Flowers, upon the Approach of my Fingers, or when disturbed by any thing else that came within Two or Three Inches of them, would in an Instant close all their Leaves together, and the Flower, Stalk and all, would shrink back into the Cavity of the Stone: Yet, if undisturbed for the Space of a few Minutes, they would again come in Sight,

* At first Sight this Species of Animals greatly resembles the Flower of the *Marigold*, but is of a paler yellow. I take it to be a sort of *Urtica marina*, of which *Gesner* has given Descriptions and Figures in his Book *de Aquatilibus*; but a Figure very nearly resembling this above described, is to be seen in *Johnston*, Hist. Nat. de *Exanguibus aquaticis*, Tab. XVIII. C. M.

and by degrees expand their Leaves, and appear in their former Beauty. From such an Appearance at first I could give it no other Name but that of a sensitive Flower; especially when I once saw several *Stamina*, but without *Apices*, rise up from the Socket of the Flower. Yet no sooner had these appeared to give me the Idea of a perfect Flower, but that replete with Animal Life, if Motion, and a Capacity of Self-preservation may be called such; these Claws, or Arms, which I must no longer call *Stamina*, darted from one Side of the Flower to another, and about its Verge, with a quick Motion, as if in Search of Prey. What further confirmed me in this Opinion, was, that I observed these Claws, when in Motion, to be jointed, and that they would often close together, as so many *Forcipes*; though their Appearance was but for a short time, soon retreating and disappearing again in the Socket of the Flower. As this seems to me, if it is allowed to be an Animal, to be its manner of taking its Prey, I leave it to the Judgment of others to consider whether, as these radiated Leaves can in an Instant close, with a strong elastic Force, to avoid Danger, they may not also when the Prey is brought within their Circle, be of Use to confine and secure it in their Embrace, till it is conveyed to the Mouth; which I suppose to be in the Socket, of what I have at first called a Flower. The Top of the Stone, out of which these seeming Flowers do grow, is covered over with Clusters of Water-bottles, that resemble unripe Grapes. Among these I found also several small blue Flowers, resembling the yellow ones in their Form and other Qualities.

—Having thus given you, Sir, the Description of this surprising Appearance, and that with the utmost regard to Truth, I beg Leave to subscribe myself,

S I R,

Mile-End, London,
Nov. 3. 1743.

Your most obedient,

Humble Servant,

Griffith Hughes.

See TAB. III. Fig. 1.

VII. *A Letter from the Reverend Mr. Roger Pickering, V. D. M. to Cromwell Mortimer, M. D. Secr. R. S. concerning the Seeds of Mushrooms.*

S I R, *Deptford, Oct. 31. 1743.*

Read Nov.
10. 1743.

THE last time but one I had the Pleasure of your Company, I took the Liberty of mentioning some Conjectures of mine upon the Propagation of Mushrooms, together with some Observations, which I had already made. I was always of Opinion, that these Plants had their Seeds, as well as others; and attributed the not discovering it hitherto, to the Shortness of this Plant's Duration, and to its succulent and loose Contexture, whereby it is liable to immediate Putrefaction from the least Alteration of Weather. I could no otherwise account for the Method made use of by the *Italians*, who make Mushroom-beds in their Cellars, with a Mixture

ture of fine Mould, and the Parings of Mushrooms laid upon Dung; and that of our Gardeners, who water their Beds with Water, wherein such Parings are soaked; but by supposing, that their Success was owing to minute Seeds lodged and retained in such Parings, and washed off by such Infusions. So also, as to the Mouldiness of old Dung and Thatch, which the Gardeners are very fond of in making their Mushroom-beds, I apprehended, that this Mouldiness was not the nutritive Juice or Salt proper for the Production of this Plant, but the Mushroom itself in its early and inceptive State. The late warm Rains have enabled me to reduce my Conjectures to a Certainty. In short, Sir, I have not only discovered, that this Mouldiness is a Collection of little Mushrooms adhering to each other by minute Fibres, or, as the Gardeners in other Cases call them, Runners; but I have had the Happiness to discover and preserve the Seed of Mushrooms.

I had prepared for my Observations, by ordering the Gardener to make a Mushroom-bed, in a well-sheltered Place, after the usual manner; which was finished about Six Weeks ago, but has not yet worked; and had charged him to let me know, if any occasionally sprung up in any Part of the Ground. Accordingly, about *Wednesday*, last Week, he informed me, that a great Plenty had appeared above-ground, among the Asparagus, and on the Grass-walks, as indeed I expected, because on *Tuesday* in the Night there had fallen $\frac{1}{3}\frac{2}{3}$ of a Cubic Inch of Rain, which, together with an unusual Height of the Thermometer, for the Season, made it the most suitable Weather for Mushrooms. I immediately chose out the most
pro-

promising Plants, which I covered with Bell-glasses, where there were several together, and the single Plants with little Hand-glasses, which I had had made for the Preservation of Wall-fruit.

• On *Friday* last, *Oct.* 28th, at Noon, I carefully gathered about a Dozen Mushrooms, of the esculent Kind, from under the Glasses; choosing such as gradually differed from each other in the Colour of their Gills, from a faint Peach-bloom Colour, to a deep Purple; flattering myself, that as I had hereby got the Mushroom, in its several States, secured by these Glasses from the Injury of the Weather, I should be able to discover the Seed.

With these I gathered several Mushrooms of another Kind, commonly known by the Name of *Cham-pignons*; which also I had secured under Glasses. With these I began, and soon found, what I suspected to be the Case, that the Gills, as they are called, are no other than *Capsulae*, or Pods for the Seed; for with one of the lower Magnifiers, and a fine Penknife, I could easily divide them from adhering to each other. This encouraged me to apply directly to the larger sort of Mushrooms; and accordingly I fixed upon one of a deep Flesh-colour, which I looked upon to be, by its Colour, in its Prime. I began with one of the Gills carefully separated from the Head, or Stool, without bruising; but could discover nothing in it like Seeds, except that, here-and-there, there were some globular dark Spots, appearing, through the Fifth Magnifier, about the Size of very small Pin-heads: But when I endeavoured, with a fine Brush, to wipe off any thing, to fix it upon a Talc, the lightest Touch reduced it to Water. Upon this, I had

recourse to a thin, but tough Filament, which was situated upon the Stalk or Stem of the Mushroom, in an exact Distance from the Head of the Mushroom, and the Mark, which the Earth round about the Stem had made. Upon this Filament appeared a fine downy Substance of a lively Brown, resembling the Down upon a Moth's Wing, but much finer. I could brush off some of this upon white Paper, without reducing it to Water; but, not having the new Apparatus for opaque Objects, (which is the only one I am without) there was nothing that appeared bold or sharp enough for me to depend upon. I had then recourse to a fine Talc in a Slider, and brushed off some of this brown Dust upon it; and, after I had applied the Second Magnifier, I was gratified with the first Sight of the Seed of Mushrooms; for I then discovered a Multitude of round, regular, transparent Bodies, bearing the same Appearance as the *Farina* of Flowers. I then applied the highest Magnifier, through which they appeared very bold, of the Size of a moderate Pin's-head.

I have endeavoured to draw a Sketch of the Mushroom, &c. in its just Proportion.

T A B. III. Figure 2.

- a. *Is the Mushroom in which I discovered the Seed in its natural Size.*
- b. *The Filament upon which the Seed was discovered, being, as I apprehend, a wise Provision of Nature, to prevent the Wind's Power over such minute Bodies as the Seeds are; for, by being placed at an exact Distance between the*
Head

Head of the Mushroom, and the Ground, it secures the Seed before the Wind's Power can affect it, unless the Wind be high; and, by another easy Fall, enables it to lodge itself safely in the Ground.

- c. *The Part of the Stem under-ground, from which the Fibres shoot, upon which the little Mushrooms, marked d, grow, appearing at first but like a white Mouldiness.*

Figure 3.

- a. b. *Animalcules of the Maggot, or Fly-kind, found in the Head and Stems of Mushrooms in a decaying State.*

Figure 4.

The Seed of the Mushroom, as it appears through the First Magnifier.

I have already trespassed so much upon your Time, and indeed, as it happens, my own, that I shall leave several Observations upon this odd Plant, as to its Potatoe-like Propagation, and the *Animalcules* which inhabit it in its decaying State, to another Opportunity; and conclude myself, with real Esteem,

S I R,

Your assured Friend,

and most humble Servant,

R. Pickering.

P. S.

I had forgot to mention under the Article *b*. Fig. 2. that the thin Filament is that to which the Edges of the Head of the Mushroom adhere, while it is, what is commonly called, a Button, and from which it separates by expanding into a Flap.

P. S.

Since I wrote the above, I have met with *Sigr. Micheli's Nova Genera Plantarum*, wherein I find the Observations which I have made upon Mushrooms, though entered upon without any Hint or Direction from him, or any other Writer, pretty near the same with his. I think it therefore a Piece of Justice, due to him, and to the Reading and Judgment of Mr. *Watson*, candidly to allow the first Discovery of the Seeds of Mushrooms to that *Italian* Botanist. It fully satisfies my little Ambition to have had the Honour of shewing them the First to the ROYAL SOCIETY of *England*.

R. Pickering.

N. B. I thought proper to print the Reverend Mr. *Pickering's* Paper on the Seeds of Mushrooms, together with Mr. *Watson's* Remarks upon it; because *Sigr. Micheli's* Book, being printed at *Florence*, is not in many Peoples Hands here; and, as that is in *Latin*, I thought it would not be disagreeable to our Gardeners to have an Account of this Discovery in *English*: Besides, it is but doing Justice to Mr. *Pickering's* Diligence in searching into the Works of Nature, since he was so fortunate as to succeed in a Discovery which had eluded many curious Botanists, and that without having taken any Hint from *Micheli*.
C. M.

VIII. *Some Remarks occasioned by the preceding Paper, addressed to the ROYAL SOCIETY by Mr. William Watson, Apothecary, and F.R.S.*

GENTLEMEN,

Read Nov. 17.
1743.

THE Reverend Mr. *Pickering* having at your last Meeting laid before you an ingenious^d Account, and shewn you, by the Assistance of a Microscope, the Seeds of Two Sorts of *Fungus's*, which were imagined, by many present, to have been undiscovered before; I hope I shall not be thought to detract from that learned Gentleman's Merit, if I mention the first Observer of those minute Bodies, although till now they never have, to my Knowledge, been shewn in *England*: For, however great my Zeal is to give the Honour of any Discovery to my own Countrymen, yet Candour will not permit me to give it them to the Prejudice of those of another Nation. It was to the late *Sigr. Micheli*, Professor of Botany at *Florence*, that the World owes the Discovery of the Seeds of Mushrooms, as well as the Flowers and Seeds of the various Species of *Lichen* or *Liverwort*: He not only saw with his Glasses, but raised from their Seeds, many Kinds of Mushrooms, as may be seen from his Experiments in Page 135. of his incomparable Work intituled, *Nova plantarum Genera*, printed at *Florence* in the Year 1729. He constantly observed the Seeds produce the same Species, as in the more perfect Plants.

A very worthy and learned Member of this SOCIETY, Dr. *Haller*, Professor of Physic, Botany, &c. in the University of *Göttingen*, in his excellent Work published last Year, intituled, *Enumeratio Methodica Stirpium Helvetiæ*, tells us, when treating of *Fungus's*, Page 34. that their Seeds are produced in the *Laminæ* of their concave Side; as he has most evidently seen in the 35th, 50th, 73d, 93d, and 107th Species mentioned in his Work; which Seeds are by Nature, when ripe, shaken from the Plants, and, being sown, propagate their Species. He likewise mentions, that the Seeds of different Mushrooms vary in their Colour, some being blue, others green, white, &c.

That Ornament of this SOCIETY, the late Mr. *Ray*, indeed, mentions a *Fungus*, discovered by his Friend Mr. *Doody*, which he calls, in his History of Plants, Vol. III. Page 21. *Fungus seminifer externe striatus*; and Monsieur *Tournefort*, in his *Institutiones Rei Herbariæ*, Page 560. takes notice of another Species of this Tribe, which he calls *Fungoides infundibuliforme semine fætum*. Monsieur *Vaillant*, in Page 57. of his *Botanicum Parisiense*, gives a Description and Figures of the Seeds of these Two Kinds. His Words are to this Purport, when translated from the *French*. “ Within the Cavity, says he, of these Plants, towards
“ the Bottom, are contained many Seeds heaped one
“ upon another, cut upon their superior Surface
“ somewhat like a Triangle, broad underneath, where
“ they are connected to a little Tendon, and are
“ whitish.” Notwithstanding the high Veneration I have for the Opinions of these able Botanists, I am satisfied the Parts of these Two Plants, so imagined,
arc

are not their Seeds, but rather their Suckers, *Stolones*; which, in most others of this Tribe, are produced from the Root; but from both these, as in many of the Kinds of *Lichen*, and in the *Dentaria bulbifera*, are produced from other Parts of the Plant. I cannot help observing, that in almost all Plants, whose Seeds are produced sparingly, or are difficult to be saved, Nature abundantly makes up that Deficiency by the great Increase of their Roots, whereby their Species may easily be propagated; as is manifest in Mushrooms, Potatoes, Crocus's, Golden-rods, Starworts, and above all in the *Corona Solis, flore parvo, tuberosa radice*, of M. *Tournefort*, vulgarly called *Jerusalem Artichokes*, the Seeds of which, from the Shortness of our Summer, having never as yet ripened in *England*. I shall only add, that although many Species of Mushrooms are eatable, and some of them better flavoured than the common Sort, the Gardeners only propagate that Sort with red Gills, called, by way of Excellence, *Champignon*, a Name given by the *French* to all sorts of Mushrooms; but some descriptive Word is added to them, whereby they may be distinguished from this. The Method of propagating Mushrooms according to the usual Practice, *viz.* from their Suckers, was first mentioned by *La Brosse*, in his Treatise *De la Nature des Plantes*, and afterwards by Monsieur *Tournefort* in the Memoirs of the *Academy of Sciences*, Anno 1707. Page 72. I am, GENTLEMEN,

Nov. 17. 1742.

*With great Respect,
Your most obedient,
Humble Servant,*

William Watson.

IX. De

IX. *De Disparitione Annuli Saturni An. 1743.
& 1744. ex Epistola a Dno Godofredo
Heinsio ad Dnum Petrum Collinsonum,
R. S. S. data.*

Read Nov. 24.
1743.

Saturnus circa finem hujus & circa medium sequentis anni suo annulo orbatus apparebit per telescopia longitudinis prægrandis. Rarum hoc est phænomenum, quod quidem singulis quindecim annis contingere deberet; interdum vero ex defectu commodi terræ situs respectu Solis & Saturni elapsis annis 30. vel 45. demum accidere solet; siquidem istud ab eo tempore, quo *Hugenius* veram annuli conditionem primus detexit, tribus circiter vicibus tantum observatum fuit. Propter emolumentum, quod ex observatione rari hujus phænomeni in theoriam annuli Saturni omnibus numeris nondum absolutam redundare potest, prædictionem ejus instituire non incongruum visum est, quæ sequentes involvit conditiones.

Duæ dantur primariæ conditiones, quas in ejusmodi prædictione attendere oportet. Concernunt istæ transitum producti plani annuli per centrum Solis & per centrum Terræ. In utroque casu Saturnus annulo orbatus apparet; in primo quidem, ex defectu sufficientis illuminationis annuli à Sole; in altero, quia margo gracilis annuli oculo directe obvertitur, cujus proinde lumen debile oculum sensibilibiter non afficit. Ambæ conditiones rarissime eodem temporis momento contingunt. Si igitur istæ diversis temporibus accidant, fieri potest ut à tempore unius conditionis usque
ad

ad tempus alterius productum annuli planum Solem inter & Terram transeat. Hoc casu superficies annuli non illuminata Terræ vel oculo obvertitur, quæ annuli disparitionem, in disco autem Saturni fasciam obscuram, efficit.

Anno sequenti 1744. productum annuli planum transit per centrum Solis d. 10. Julii hor. 22. styl. nov. in meridiano Parisiensi; per centrum Terræ vero d. 25. Augusti hor. 3. A termino primo usque ad alterum annuli planum productum Solem & terram semper interjacet; verum post d. 25. Augusti illuminata annuli superficies terræ iterum obvertitur. Hoc modo ex his conditionibus annulus Saturni disparebit d. 10. Julii, hor. 22, nec, nisi post hor. 3. d. 25. Augusti, in conspectum iterum redibit. Statim post disparitionem fascia larga obscura discum Saturni supra ventrem ejus (vel infra centrum in tubo astronomico) occupabit, quæ successu temporis arctior evadit, & tandem circa d. 25. Augusti disparet. Si ejusmodi fascia per centrum disci Saturni transiens circa d. 25. Augusti adhuc conspici posset, indictio foret, anulum crassitie sensibili prædictum esse.

Prædicta disparitionis & apparitionis annuli tempora firmis quidem innituntur principiis; coelo tamen non respondebunt. Annulus citius, quam d. 10. Julii, disparebit, & serius, quam d. 25. Augusti in conspectum veniet. Neutrum caret ratione. Transitus scilicet plani producti annuli per Solem non sola est disparitionis causa. Requiritur præterea sufficiens annuli à Sole illuminatio, ut iste in conspectum redire possit, quæ vero certam Solis super plano annuli elevationem supponit. Hanc $\frac{1}{4}$ gradus æqualem, vel etiam minorem, statuit *Maraldus*. Sumta priori, annulus 14
dies

dies ante stabilitum terminum diei 10. Julii, vel die 26. vel 27. Junii ultima vice sufficienter illuminatus & disparitioni proximus erit, quæ lumine successive decreſcente paulo poſt eveniet. Obſervatio de prædictionis eventu decedet, & terminum anticipatæ diſparitionis certius definiet: in hoc enim prædictionis genere error aliquot dierum vix evitari poteſt, tum quia Sol altitudinem ſuper plano annuli per paucos dies ſenſibiliter non mutat, tum quia terminus ſufficientis illuminationis nondum exacte definitus eſt; præſertim vero, quia diverſitas telescopiorum in obſervatione hujus phænomeni adhibitorum, & diverſa oculorum acies diſcrepantiam non exiguam producere valent. Quamobrem ſtatueri licebit, annulum circa finem Junii vel initium Julii diſparituum eſſe.

Eodem modo annuli quidem aſpectus ceſſabit, quando planum ejus productum per centrum terræ vel oculum tranſit; diſparitio tamen ejus locum quoque habebit, quando oculis ſuper illuminata annuli ſuperficie ſufficienter elevatus non eſt, quippe quo caſu iſta radios Solares nimis oblique ad nos reflectit, ut oculorum ſenſum movere nequeant. Supponi autem ſolet, elevationem oculi ſuper plano annuli uni gradui æqualem requiri, ut anſæ Saturni in conſpectum venire poſſint; quo poſito, annulus non d. 25. Auguſti, ſed d. 10. Septembris demum apparebit. Aſt terminus hic ſufficientis elevationis oculi ſuper plano annuli minoris adhuc eſt certitudinis, quam ſupradictus illuminationis ſufficientis terminus; unde non mirum eſt, ſi etiam hic eventus à prædictione aliquot diebus aberraret, præſertim cum variatio, quæ ex telescopiorum & oculorum diverſitate oritur, nondum ſatis cognita ſit. Iſte autem terminus exactiori modo determinabitur

bitur ex observatione, cui casus exoptatissimus in præfenti velificatur.

Circa finem scilicet Novembris anni hujus 1743. terra valde appropinquat ad superficiem annuli productam, quæ à Sole illuminatur, ita ut terra die 22. Novembris non nisi uno gradu super istâ elevata sit. Inde terra adhuc proprius accedit ad dictam superficiem usque ad d. 8. Decembris, quo illi proxima est sub altitudine $34\frac{1}{2}$ minut. Terra deinceps recedit iterum à prædictâ superficie, super quâ d. 29. Januarii 1744. altitudinem unius gradus nanciscitur, quæ successu temporis adhuc crescit. Si igitur terminus sufficientis elevationis oculi super plano annuli uni gradui æqualis pro certo assumatur; statim post d. 22. Novembris hujus anni ansæ Saturni disparere, nec nisi die 29. Januarii sequentis anni in conspectum redire debent. Sin autem oculus elevatione tantum dimidii gradus super plano annuli opus haberet, ut conspectus annuli concederetur; nulla ansarum Saturni disparitio circa finem anni currentis futura esset; sed annulus demum circa finem Junii vel initium Julii sequentis anni oculis nostris ex ratione supra allegatâ subduceretur. Hoc modo terminum prædictum observatio optime definit, si advertatur, an & quando annulus circa finem hujus anni dispareat, & circa initium sequentis iterum appareat. Eo casu, quo annulus plano suo producto terræ propinquior disparere vel apparere solet, observatum fuit, Saturnum ansam utramque non eodem tempore perdere vel recuperare; necnon ansas curvatas & inæqualis magnitudinis apparere. Hæ circumstantiæ attentionem observatoris merentur, ut conjecturæ robur addi possit, quâ omnes annuli partes non in eodem plano sitas esse astronomi collegerunt.

Si jam conditiones hætenus enumeratas ex assumtis principiis breviter repetere velimus; sequentia ordine temporis phænomena obvia erunt. Ansa Saturni hoc anno usque ad finem Novembris conspicua tunc disparebunt, nec nisi circa finem Januarii sequentis anni in conspectum redibunt. Ab hoc tempore Saturnus ansas suas retinebit usque ad finem Junii, vel initium Julii, quo tempore istæ, lumine successive decrescente, disparebunt. Phasis rotunda Saturni tunc conspicua erit usque ad initium Septembris, circa quod adpectus annuli redibit, per quindecim annos deinceps duraturus. Per totum tempus situs Saturni respectu Solis observationibus favebit, si eam excipias, quæ reditum annuli mense Septembris anni sequentis concernit: Hoc enim tempore Saturnus crepusculo oculis eripietur, conjunctioni scilicet cum Sole proximus, quippe quæ die 14. Septembris continget. Vale.

Dabam Petripoli, d. 13. Septembris 1743.

X. *An Abstract of a Natural History of Greenland, by Hans Egedius, intituled, Det gamle Gronlands Perlustration, eller Naturel-Historie, af Hans Egede. Kjøbenhavn, 1741. 4^{to}. Communicated by John Green, M. D. Secretary of the Gentlemens Society at Spalding.*

The Title Page is as follows.

Read Dec. 8.
1743.

A Description of old and new *Greenland*, or a Natural History of old *Greenland's* Situation, Air, Habitude, and Circumstances.

The Beginning and End of the old and new *Norwegian* Colonies. The present Inhabitants, their Original, Manners, Living, and Employments.

The Products, as Beasts, Birds, Fish, &c. With a new Chart, and several Copper Plates. By *Hans Egedius*, formerly Missionary in *Greenland*, and now Superintendant and Professor in that Language.

It is dedicated to the Prince Royal of *Denmark*, &c.
Imprimatur Marc Woldike.

CAP. I. *Situation, Climate, and Soil*, to p. 4.

Greenland lies about 160 *English* Miles West from *Iceland*, begins at 59 Degrees, 40 Minutes North Latitude.

Its East Side stretches to *Spitzbergen* 78 to 80 Degrees Latitude, and believed to be an Island separate from *Greenland*.

Its West Side is known to 70 Degrees Latitude. If *Greenland* is an Island, or joined to other Countries, it is not known for a Certainty, but probably joins to *America* on the North-west Side: For between *America* and *Greenland*, stretches the *Fretum*, or Bay, called in the Sea-Charts *Davis's Streights*, which is navigated by them and other Nations on Account of the Whale-fishery, but to the Bottom of this Sound no Ship has ever been.

Greenland is an high rocky Country, which is always covered with Ice and Snow, which never thaws except near the Sea. The highest Land can be seen 80 *English* Miles from the Sea. The whole Coast is fortified with large and small Islands. It has several Firths or Rivers, which run a long Way within Land; amongst which is *Baal's* River, where the first *Danish* Colony was fixed in 1721. which runs 80 Miles within Land. That in all Sea-Charts called *Forbisber's-Streight*, also *Baer-Sound*, which are said to make Two large Islands at a Distance from the main Land; but, in Reality, I did not find them so.

CAP. II. *Colonies and Conversion*, to p. 23.

Greenland was first discovered by the *Norwegians* and *Icelanders*; and the brave *Raude*, who first discovered it in 982. praised it, and persuaded several of his Countrymen to inhabit it; and at the Instance of *Oluf Tryggesson*, first Christian King in *Norway*, carried a Priest with him, who taught and baptized all the Inhabitants; and from time to time *Greenland* multiplied into new Colonies, many Churches and Abbeys were built, Bishops and other Teachers provided for: But the *Norwegians* were not the first
In-

Inhabitants; for they found wild People on the West Side, who without doubt were originally *Americans*. The present Inhabitants probably are a Race of the *Schrellingers*. In 1545. *Dithmar Blefken* reports a Monk, with his Bishop, sailed to *Norway*, lived to 45 Years in *Iceland*: And he reports, that a *Dominican* Cloister was in *Greenland*, called *St. Thomas*. But this is proved false by *Arngrim*.

Mogens Heinson was sent to find out *Greenland*, and was obliged to return, because his Ship was stopped (as he imagined) by magnetical Rocks under Water, although the Wind was favourable; but the real Magnets probably was the Current, which is so strong at *Staton Point*, a Ship under full Sail with the fairest Wind fails slow.

In 1721. a Company of Traders was set up in *Bergen*, with a Royal Privilege, when King *Frederic* resolved to begin a Colony at 64 Degrees, wherewith I and my Family went, and continued 15 Years. Our Design was to find the Eastern District, as the best: A *Hollander* affirmed some of their Ships had been there, and found the Land free from Ice in 62 Degrees. This I found to be true in 1736. on passing *Staton*, *Huck*, and *Cape Farewel*, near the Land, then free from Ice on the Coast, which was not usual: But as it is seldom that Ships can come with Safety to the East Side, it is most convenient with small Boats through the Openings near the Main, where the Current setting South-west prevents the Ice from fixing.

CAP. III. *Natural Products*, to p. 27.

In the Bay of *Hope* there are many good Places for feeding of Cattle, with proper Ground for Tillage, and good Water : No Trees, except within the Rivers, only Brush-wood : Juniper-bushes abound here, whose Berries are the Size of the largest Pease. There are divers Plants here, as *Angelica*, *Rosemary*, *Scurvy-grass*; and a Grass with yellow Flowers, whose Root smells like Roses in the Spring.

In 60 and 65 Degrees, the Country is best, and *Barley* will ripen there : *Turneps* and *Colwort* grow well ; especially the first, which are large, and of a sweet Taste.

There are Rocks which produce *Verdegrise*, as also *Sulphur* or *Brimstone*, *Marcasite* ; and I found on an Island one of a yellow brown Sand, having *Cinnabarine* red Veins. There are whole Mountains of the *Asbestos*. There is found a grey Stone, or Bastard Marble, of different Colours. The Sea produces several sorts of *Conchs* and *Mussels*, also divers sorts of *Corallines*.

CAP. IV. *Air and Weather*, to p. 32.

The Summer here lasts from *May* to *September* : The Cold at 64 Degrees is moderate, but at 68, &c. extreme, and will freeze Brandy.

The Land is constantly covered with Ice and Snow, except near the Sea, and in the Rivers. Although the Summer oft-times is warm in *Greenland*; it seldom or never thunders, &c. The *Aurora Borealis* is so strong here towards New Moon in clear Weather, as you may read by it.

CAP. V. *Beasts and Birds, Hunting and Fowling,*
to p. 36.

Greenland produces *Bears*, which live on the Ice, and are dextrous at catching *Otters*, *Seals*, &c. *Rain Deer* are in great Plenty. *Hares* are very large, good and white all the Year. There are plenty of *Foxes*. They have *Dogs*, none of which can bark, only howl.

Their Birds are the *Ryper*, or *Wood-Partridge*, *Ravens*, *Eagles*, *Falcons*, *Sparrows*, *Goldfinches*, &c.

The *Mosquitoes* are very troublesome in *July* and *August*.

CAP. VI. *Fishes and Amphibious Animals, and Fishing; Whales, Narval or Sea-Unicorn, and Sea-Birds,* to p. 55.

The Sea produces *Whales*, the *Fin-fish*, which live on a kind of *Louse*, brown-coloured, who moves so slow, that he may be taken by Hand. This Creature is oily, and, when rubbed with the Fingers, produces *Train*.

There is another sort of *Whale* in these Seas, called *North Capers*, which feed on *Herrings*; as also the *Sword-fish*, who is the *Whale's* greatest Enemy; and when he kills one, eats nothing but his Tongue, leaving the rest to the *Shark*, *Walross*, and Birds of Prey. In these Seas are *Cachelots* or *Pot-fish*, a sort of *Whales*, their Length 50 to 70 Feet. The *White-fish* are likewise in these Seas, like a *Whale*, but without Fins on the Back. There is likewise a small *Whale* produced here called *Butts-kops*; as also *Unicorns* of the *Whale* kind, which they call *Narval*:
Their

Their Horn, as some Authors affirm, are not Teeth, because its Root is not in the Jaws, but goes a long Way into the Head. The *Niser*, or *Porpoise*, are also in these Seas; as also the *Walrofs*, shaped like a *Seal*, but much larger; his Flesh is like fat Pork: His irreconcilable Enemy is the white *Bear*. There are several Sizes of *Seals*, but of the same Shape, except the *Klap-myfs*, which has a cartilaginous Hood, which covers his Eyes. There are other Fish, as *Sharks*, *Holly-butts*, *Red-fish*, *Trout*, *Salmon*, *Bull-heads*, *Stone-biters*, *Smelts*, *Whitings*, *Herrings*, and a Fish like a *Bream*, with Pricks on its whole Body. There are *Mussels*, and some large ones that produce the Pearl: Here also are *Shrimps*, *Crabs*, &c.

Amongst the Sea-birds are the *Edder*, *Ducks* of Three Kinds; as likewise the *Alker*, and the *Tornauviarsuk*, which has beautiful Feathers, and the Size of a *Lark*: There also are *Geese* here. *Greenland* produces *Maws*, *Redhanks*, *Cormorants*, *Lunders*, *Parrots*, *Sharvers*, *Tersters*, *Angle-tasters*, *Snipes*, &c.

CAP. VII. *Imployments and Utenfils*, to p. 63.

The Imployments of the *Greenlanders*, on Shore, are to shoot *Rain-Deer*; and at Sea to catch *Whales*, *Seals*, *Birds*, &c. The Bow is about Six Feet long, of tough *Fir*, which they bind round with *Deer* Sinews: The Point of the Arrow is pointed with Iron or Bone. All the sort of Fish they catch, and cannot eat fresh, they dry against Winter.

The Boats are of Two sorts; one used only by the Men, about Three Fathom in Length, their Breadth about 19 Inches, with an Hole in the Middle, not
larger

larger than one Man, close-laced, can thrust himself into; with these Boats they are able to row 72 Miles a Day, using only one Oar.

CAP. VIII. *Manners and Habitations*, to p. 66.

Their Houses are of Two sorts, Winter and Summer: The former are made of Turf and Stone, from Four to Six Feet high, flat-roofed; on one Side are the Windows, made of bleached *Seal-guts*, *Holly-butt Maws*, sown together, and are sufficiently transparent: Their Doors are very low, they creep in on their Hands and Knees. Their Summer-houses are made by raising Poles, which they cover with young *Seal-skins*.

CAP. IX. *Shape, Constitutions and Tempers*, to p. 68.

The Inhabitants of the Northern Parts are troubled with Dysenteries, Bloody-fluxes, &c. They have seldom any contagious Distempers: They use no Medicines; and, instead of Remedies, their Conjurers mumble over their Bodies some strange Jargon. Wounds they sew up; Cataracts on the Eyes they take off as follows: They insert a crooked Needle under the Skin, and with a Knife raise it up, and draw it off safely. When their Children are troubled with Worms, the Mother puts her Tongue up the Fundament, to kill them.

CAP. X. XI. and XII. *Of their Customs, Capacities, Cloathing, Diet, and Cookery*, to p. 77.

CAP. XIII. *Marriages and Education*, to p. 82.

They have riotous Assemblies, in which it is reckoned good Breeding, when a Man lends his Wife to a Friend. None come to these but married People. The Women esteem it a piece of Fortune when they have to do with their Prophet, and the Husbands pay them for the Honour; especially if they prove with Child, their own Endeavours having been fruitless.

The Women, as soon as delivered, go immediately about their usual Work. The Navel-string must not be cut by a Knife, but a *Muscle-shell*, or bit off; when dried, it is used as a Charm. They hold a Piss-pot over the Womens Heads whilst in Labour, thinking it to promote hasty Delivery: They seldom bring Twins, but often Monsters.

CAP. XIV. *Manner of burying their Dead, and preserving their Corpses under Tumuli of Stones.*CAP. XV. *Games, Poetry, Music, and Dancing*, to p. 93.

They have several Diversions amongst them, as Singing, Dancing, in which they challenge one another. They play likewise at Foot-ball: Thus, they say, the deceased Souls play in Heaven with a *Wal-rofs's* Head, which is performed when the *Aurora Borealis* appears.

CAP. XVI. and XVII. *Language and Vocabulary*,
to p. 105.

At Page 86. the Author presents his Reader with a *Greenland Ode*, much like *Lapland Poetry*.

Their Language has no Affinity with any known *European* one: Few Words are like the old *Norwegian*. It is difficult of Pronunciation, as most of their Words are Gutturals. It has not the Letters c, d, f, g, x.

CAP. XVIII. and XIX. *Present State of Trade in Greenland, and of Religion there*, to p. 120.

Their Religion consists in nothing more than superstitious Ceremonies.

CAP. XX. *Astronomy*, to p. 125.

The Sun, Moon, Planets, and other Stars, they imagine had their Beginning from their Forefathers, and were formerly People by a singular Manner taken up to Heaven. They are of Opinion, that when the Moon does not appear, or is dark, it is seeking her Sustenance on the Earth: And they say further, that it sometimes comes down, and makes Whores of their Women; for which Reason none dare lie on their Backs, before they spit on their Fingers, and stroke it over their Bellies; and young Girls dare not stare at the Moon, for fear of conceiving by her.

CAP. XXI. *Considerations of the Reverend and Learned Author, for promoting the Success of his Mission, and the Salvation of the Greenlanders*, to the End.

XI. *Some Observations on a Polype dried.*
By Mr. Henry Baker, F. R. S.

Read Dec. 8.
 1743.

Apprehending that if a *Polype* could be dried, and well extended before the Microscope, some Particulars in its Structure might be distinguished better than when we view it alive, and in Water, I applied myself to attempt the doing it: And, after many Trials, (which were rendered fruitless by the Minuteness and extreme Tenderness of the Arms and other Parts of this Animal, that contract as soon as taken out of Water, and so cling together, as to become inseparable afterwards, without being torn to Pieces) I happened, at last, to hit on a Method of performing the Operation perfectly; which Method I shall here subjoin, as taken from my Essay on this Creature lately published *.

The Method of drying Polypes for the Microscope.

I chuse a *Polype* to my Mind, and put it in a small convex *Lens* with a Drop of Water; where, when it is extended, and the Tail fixed, after pouring off a little of the Water (if the Quantity seems too much), I plunge it, *Lens* and all, into Spirits of Wine, in the Bowl of a large Silver Spoon. Hereby it is instantly killed; the Arms and Body contracting, sometimes more, sometimes less, at the same time. I then rub it gently in the Spirits with a very small soft hair Pencil, to clear away its Lice, which may be seen to fall off, and lie dead at the Bottom of the Liquor.

* *Natural History of the Polype*, p. 84.

Thus

Thus far the Business seems pretty easy ; but all the Skill I am Master of could never enable me to take a *Polype* out of the Spirits, and extend its Body and Arms on a Talc, though I have destroyed great Numbers in attempting it ; for the Parts immediately cling together, in such a Manner, that it is not possible to separate them again, without tearing them all to Pieces.

I bethought myself, therefore, of adjusting them upon the Talc, whilst in the Spirits; and, to effect this, I slip a Talc under the *Polype's* Body lying in the Spirits; and, displaying its Arms, &c. thereon with my Pencil, by the Assistance of my Nippers I lift the Talc, and the *Polype* upon it, gently out of the Spirits: Then, holding it in my Nippers with my Left Hand, I dip my Pencil in the Spirits with my Right, and therewith dispose the several Parts to my Wish as near as I am able ; at the same time wiping away the Lice with my Pencil, if any are to be seen upon the Talc.

When all the Parts are rightly extended, I lay it carefully to dry, which it does very speedily, leaving the *Polype* sticking to the Talc in the Manner it was disposed.

The chief Difficulty now is over ; but some Caution is still needful to secure it safely in a Slider: For, if another Talc be laid upon it in the common Way, all our Labour will be rendered fruitless, by its being broke and spoiled. To prevent this Misfortune, as soon as the Talc, whereon the *Polype* sticks, is let down into the Hole of a Slider, I cut Three little flat Pieces of Cork, about the Bigness of Pins-heads, and the Depth of the *Polype*, and gum them in a triangular Position, partly on the Edges of the
said

said Talc, as it lies in the Hole, and partly to the Ivory Sides of the Hole itself; by which means the upper Talc being kept from being able to press upon the *Polype*, it may be put on and fixed down with a brass Ring, without any Fear of hurting it.

If you intend to dry a *Polype* in its contracted State, it may be put directly into the Spirits without using any *Lens*; but if you desire it extended, you will find the *Lens* quite needful.

Vinegar, Water wherein Salt is dissolved, or Spirit of Wine, kills a *Polype* immediately: But Spirit of Wine is fittest for the Purpose, as it gives a greater Firmness to the Parts, dries away from the Talc soonest, and leaves no Soil or Smear behind it, as the others do.

Having now the Honour to shew the Gentlemen here present a *Polype* prepared after the above Manner, and inclosed in a Slider between Two Pieces of *Isinglass*; I shall beg Leave to point out what may particularly be learned thereby, and give a Drawing of the same *Polype*, as viewed by the Microscope, [see TAB. III. Fig. 5.] in order to make the following Observations more intelligible and satisfactory.

First, As the Body thus dried exhibits a Reticulation of minute Vessels, which appear every-where most curiously interwoven, we may reasonably suppose they serve as Veins and Arteries, through which some kind of Blood or Juices circulates, as in other Animals: But we cannot distinguish such Blood or Juices circulating in the living *Polype*, or discern any thing like Vessels, though now they are so apparent.

Secondly, The *Anus* of the *Polype* may be discovered very plainly in this dried Object; whereas in a living

living one it requires much Attention to see it in a satisfactory Manner.

Thirdly, The Mouth, or Opening between the Arms, appears here like the Mouth of a Sack or Bag, which indeed the Body does not badly represent.

Fourthly, By observing the Arms thus dried, we obtain a clear Idea of the Means whereby this Creature catches fast hold of its Prey, the Moment of its touching it, and before it can bring its Arms to clasp about it: For we plainly see here, that the Arms are thick beset with Hairs, or rather sharp Hooks, which possibly are moveable, and can strike easily into the Body of a tender Worm. But these Hooks or Hairs are not visible in the living Animal; being then, perhaps, some-how or other generally drawn in, or laid flat and close along the Sides of the Arms, as I have seen them in some sorts of Star-fish. Besides, the Water wherein we are obliged to view a *Polype*, when alive, will not permit so strict an Examination as it can now be brought to.

XII. *A Catalogue of the FIFTY PLANTS; from 'Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1740. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præf. By Joseph Miller, Apothecary, Hort. Chel. Præf. & Prælec. Botan.*

Read Dec. 15. 901.
1743.

A Canthus sativus, vel mollis
Virgilii. C. B. 383.

902. Acanthus aculeatus. ib.

903. Acanthus spinis mitioribus. *Pluk. Almagest 6.*

904. Antirrhinum angusto folio Linariæ eleganter
variegato, flore rubro, rictu luteo. *Boerb.*

905. Anthora. Off. *Aconitum salutariferum sive An-*
thora. C. B.

906. Aphaca. Park. *Lathyrus luteus annuus foliis*
Convolvuli min. Mor.

907. Brassica campestris Perfoliata alba. C. B.

908. Cannabis sativa alba. Off.

909. Centaurium majus foliis in plures lacinias di-
visis. C. B. Off.

910. Clinopodium Orientale hirsutum, foliis inferi-
oribus Ocimum, superioribus Hissopum refe-
rentibus. *Tour. Corr.*

911. Cassida Alpina supina magno flore. ib.

912. Colutea vesicaria. C. B.

913. Cochlearia minor rotundifolia nostras. *Raii*
Syn.

914. Cro-

914. *Crotolaria Americana* frutescens. flore luteo,
ramulis alatis, folio mucronato *Ind. Hort.*
Chel.
915. *Dracocephalon Americanum*. *Breyn. Prodrum.*
916. *Doronicum Romanum*. *Off.*
917. *Delphinium*, sive *Consolida regalis* flore rosco.
Tourn.
918. *Ephemerum Virginianum* *Tradescanti* flore pur-
purco. *Park. Par.*
919. *Eupatorium Americanum* herbaceum *Melissæ*
folio villosum. *Houft.*
920. *Ferula major*, seu *foemina Plinii*. *Morr. Umb.*
921. *Fraxinella* flore albo, sive *Dictamnus albus*. *Off.*
922. *Fumaria siliquosa* sempervirens. *Cornuti.*
923. *Galega quinquefolia* floribus parvis rubris.
Houfton.
924. *Gingidium Rawolfii* foliis *Foeniculi*. *C. B.*
925. *Harmala*. *Off. Ruta quæ dici solet Harmula.* *J. B.*
926. *Herba Paris*. *Off.*
927. *Hedysarum clypeatum* flore suaviter rubente.
Hort. Eystet.
928. *Hieracium hortense* floribus atropurpureis. *C. B.*
929. *Jacobæa maritima*. *Ib.*
930. *Lathyrus perennis latifolius*. *Ib.*
931. *Lathyrus sylvestris Dodonæi*. *Park.*
932. *Lathyrus Siculus* flore odorato magno. *Boerb.*
Ind. 159.
933. *Lychnis multiplex* flore purpurco. *C. B.*
934. *Mentha rotundifolia*, odore *Menthæ Romanæ*
verticillis minimis. *Rand.*
935. *Menthastrum*. *Off. Menthastrum spicatum folio*
longiore candicante. *J. B.*

936. *Millefolium purpureum vulgare.* *Raii.*
 937. *Nigella arvensis cornuta.* *C. B.*
 938. *Ostrya Americana fructu Lupuli.* *Ind. Hort. Chelf.*
 939. *Parietaria Polygoni folio canescens.* *Tourn.*
 940. *Polypodium sensibile.* *Munting. Hist.*
 941. *Pseudo-Dictamnus verticillatus inodorus.* *C. B.*
 942. *Senna Italica, five foliis obtusis.* *Ib.*
 943. *Sideritis Romana utriculis spinosis.* *H. Lud. Bat.*
 944. *Thlaspi arvense minus, luteum.* *Park.*
 945. *Thlaspi. Off. Thlaspi arvense siliquis latis.* *C. B.*
 946. *Urtica pilulifera Parietariæ foliis.* *Hort. Reg. Paris.*
 947. *Valerianella, semine stellato.* *C. B.*
 948. *Vulneraria rustica.* *J. B. Anthyllis leguminosa. Raii Syn.*
 949. *Vulneraria flore purpurascente.* *Tourn.*
 950. *Vulneraria pentaphyllos.* *Tourn. Anthyllis leguminosa vesicaria Hispanica.* *Park. 1094.*

XIII. *Epistola Johannis Bevis, M. D. ad Gul. Jones, Armig. R. S. S. de Transfibus Mercurii sub Sole, Oct. 31. 1736. & Oct. 25. 1743.*

Read Dec. 15. 1743. **M**OREM tibi gerens, vir amicitie, sine, observationes meas de Mercurio, his sublati ex schedis diariis transtuli, mancas quidem invidia cœli, dignas tamen fortasse, ne percant; cum cuiquam in Britannia nostra meliorem sortem con-

contigisse non adhuc audiverim; cumque contactus pene momentanei, quos subnotare licuit, longitudinibus locorum designandis poterint optime inservire.

Ab Halleio invitatus, qui me sibi adiutorem fore dignatus est, Octob. 31. 1736. die nempe quo celebranda esset Mercurii cum sole copula, Grenovicum diluculo me contuli. Coelum ab exortu Solis fuit pulcherrimum, sed ventus haud valde remissus. In eodem conclavi mecum versabatur ipse Halleius, qui horologio adesse voluit; dum ego tubum viginti quatuor pedum dirigendi gererem curam. Circa octavam horam ad observandum me comparavi; quippe verebar, ne, fallente calculo, ingressus præterlaberetur: at nil in Sole visum prætermaculas. Illico coelum obductum est, nec ita multo post plane obscurum redditum. In decimam fere matutinam protracta est dies, cum paulo dehiscerent nubes, & Mercurii sub Sole apricantis intervidendi prima nata est occasio, momento citius densissimis nubilis abrepta. Haud diu invigilaveram, cum Mercurium iterum viderim, & Halleio ostenderim; scilicet in facie Solis, cui ibi bis antea apparuerat. Longa deinde & vere maligna nubium successio; sub Meridiem autem serenari coeptum est; & quidem solem culminantem quadrante murali maximo Halleius observavit. Revixit jam plena spes Mercurii exeuntis observandi; & quidem, cura reassumpta, notavi quæ sequuntur.

Temp. Appar.

* D. H. ' . ''.

Oct. 30. 23. 50. 45. Centrum Mercurii a Solis limbo proximo distat 1'. 08''. metiente Micrometro.

D. H. ' . ''.

Oct. 31. 00. 02. 39. Mercurius quasi propria diametro a Solis margine distinctus.

07. 04. Centrum egressum æstimatione oculi.

08. 33. Contactus exterior, cœlo admodum sereno.

Tantilla autem fuit Mercurii diameter, ut, micrometro capta, vix decem secunda scrupula superaret.

1743. *Octob.* die 25. quo Mercurius rursus in Solem incurrere debuit; Londini, idque in *Edificiis Beaufortensibus*, semiminuto circiter temporis a regia specula ad occasum sitis, illi invigilare decreveram. Constitutio cœli fuit ipsissima quæ in superiore transitu, si non vehementior flaret Africus; ex quo, non parum, incommodi ob vacillationem telescopii, validissimo fulcro licet innixi. Micrometrum adhibere plane arduum fuit; nec quicquam plus semel mensuravi, idque vix tute, facta collatione ad intercapedinem eodem fere momento, in clauso conclavi, exactissime Grenovici captam; quare suppressendum arbitror. Horologium numeravit D. Jeremias Sisson, me in Solem intento. Hora octava matutina nil in Sole conspicuum, quem paulo post plurimæ nubes occuparunt. Decima cum semisse vix præterlapia est, cum Mercurium primo detexerim, dimidio itineris jam pene confectio. Nubes rursus exceperunt Solem, qui tamen instante meridie rediit & inclaruit, quo Mercurii Solisque adscensionalis differentia innotuit: Tribus etenim verticalibus filis in meridionalis transitorii foco collocatis, *Oct.* 24^{d.} 23^{h.} 57'. 48'', T. App. ad primum horum Solis præcedens

cedens limbus est devolutus, elapsisque 25'' Mercurii centrum ibidem exstitit. Rursus ingruébant nubes cum tristissima facie coeli, neque Mercurii iterum conspiciendi ulla spes affulsit; & quidem pauca decedente pluvia. Tubum cum sustentaculis ab aperto intus transferendi cogitabam, & observando finem imponere, cum effringi coeperint nubila, & ab eo tempore rarefcere. Lætus itaque ad negotium reversus, Mercurium in splendidissimo Sole summe nigræntem contemplatus sum, & ultimas phases exacte defini: en ea quæ chartis mandavi.

Temp. Appar.

D. H. ' . ''.

Oct. 25. 00. 58. 34. Inter Solis Mercuriique limbos distantia Mercuriali diametro æqualis quamproximum.

01. 00. 33. Contactus ultimus interior.

01. 25. Centri egressus, iudicio oculi.

02. 16. Contactus ultimus exterior.

Pridie, paulo post meridiem mensurata est diameter Solaris 32'. 27''. tubo 12 ped. micrometro armato, & optimæ notæ, eodem nempe quo in hisce usus sum.

Haftenus de proprio penu. Sed neque prætereunda est observatio circa initium insignis a *D. Bird* peracta, in vico dicto *Surrey-street*, a nostro ad orientem secundo minuto temporis cum semisse remoto. Is nimirum, telescopio catadioptrico multum ampliante, exile admodum lucis filamentum inter Solis & Mercurii jam ingressi limbos deprehendit, vix decimæ parti, ut aiebat, Mercurialis diametri æquale; idque 8^h. 30'. 56''. scilicet 8^h. 30'. 54''. $\frac{1}{2}$. in ædificiis Beaufortensibus, uti ex horologiis probe a meipso in-

invicem comparatis innotuit; unde totalem ingressum in ædificiis Beaufortensibus ad $8^h. 30'. 40''$. quamproxime referre haud iniquum puto.

Liceatne demum ex præmissis integrum transcursum, quasi in ædificiis Beaufortensibus visum, in hunc modum contexere?

Temp. Appar.

D. H. ' . ''.

Oct. 24. 20. 28. 57. Contactus primus exterior.

29. $48\frac{1}{2}$. Centri ingressus.

30. 40. Contactus primus interior.

Oct. 25. 01. 00. 33. Contactus ultimus interior.

01. $24\frac{1}{2}$. Centri egressus.

02. 16. Contactus ultimus exterior.

Quoad temporis momenta in utroque transitu notata, minime hærendum censeo, cum a Sole meridiano utrobique capto rem extra dubitationis aleam poni nemo negaverit.

Valeas:

Nov. 7. 1743.

Tui observantissimus,

J. Bevis.

XIV. *Part of a Letter from M. Geoffroy, F.R.S. and Member of the Royal Academy of Sciences at Paris, to Sir Hans Sloane, Bart. late President of the ROYAL SOCIETY, concerning a Child of a monstrous Size.*

Read Dec. 22.
1743.

Normandy has furnished us some Years ago a Child, monstrous by its Size, and a Strength which its Age could not naturally afford. It was born at *Roüan*, and is a Prodigy of Virility, of Three Years and Two Months of Age, perhaps One Month older, and is now in the Hospital at *Roüan*. It has a very large Neck, the Breast very broad, and the Belly bigger than in its natural State. The upper Part of the Thighs is a little thickish, the rest is conformable to its Age. He has Hair only about the Privy Parts; the *Penis* is Three Inches long when there is no Erection, but of Six when there is any. They have found him to have Emissions. The Fact is very true, and M. *Le Cat*, F.R.S. a Surgeon at *Roüan*, has fully traced it out.—

XV. *Two remarkable Medical Cases, one of an Extraordinary Hæmorrhage, the other an Ascites cured by Tapping; communicated by Henry Banyer, M.D. Extra-Licentiate of the College of Physicians, London, to C. Mortimer, M.D. Secr. R. S.*

Read Dec. 22.
1743.

IN the Month of *January* 1729. *Daniel Goddard*, a Gardener, about the Age of Twenty-four Years, at *Wisbech* in the Isle of *Ely, Cambridgeshire*, happened of a slight Puncture from a rusty Nail in the Sole of his Right Foot. And, notwithstanding there was not wounded any Tendon, or Blood-vessel, larger than small Branches of Veins, the whole Foot was immediately swelled to a very unusual Degree, without any Fever, or other apparent Cause for it. It was also attended with great Pain, and an extraordinary Pulsation upon the Part, as in Wounds of Arteries; and so distended as if the Blood would burst out of its Vessels.

Accordingly, after Two Days, upon opening a superficial Sinus, to enlarge the Wound, there rushed out immediately such an obstinate Flux of Blood, as would not yield to any styptic Means, longer than the Bandage was holden on by some strong Hand. And, although, by this Incision, no Vessels were wounded, but Capillary Veins; yet this *Hæmorrhage* continued to show itself as violent as at first, for Six Days successively, whenever the necessary Means were relaxed. Upon which, for the sake of *Revulsion*, the Patient had

had a Vein opened on the Arm of the opposite Side; and it had such a sudden and surprizing Effect, that the Flux of Blood in the Foot instantly ceased, and the Wound healed very soon without any further Trouble; but the Flux of Blood, consequent upon Venesection, became equally as difficult to restrain, as that in the Foot, for the Space of Four Days; all which time it would have continued to flow most violently without the strictest Bandage, as the same Care of the Hand, as before. Perhaps the Period of this *Hæmorrhage* might have been much longer, if I had not suffered the Ligature on the Arm to be loosened now-and-then, as I judged the Redundancy of Blood required, for the sake of some Evacuation, at each time. After the Bleeding, he soon recovered his Strength, so as to do his Business in the Gardens; and continued very well till the Month of *March* 1730. About the Middle of this Month, he complained of Sleepiness, and a particular Heaviness all over his Body; which was followed, in Three Days time; by a violent *Hæmorrhage* from the *Nose*. This Flux, in spite of all Means being tried, except Venesection, continued Seven Days, and could never be totally stopped, all this time, for one Hour together. He recovered again in a very short time, and was able to work in the Summer-season, without any Complaints, till *October* following. Then the *Hæmorrhage* returned again at the *Nose*, as before, with all the same Circumstances, and in Defiance of all Endeavours, continued the Period of Seven Days. Thus it returned in like manner of Bleeding, by *Stools*, in the Middle of *March* 1731, and continued to discharge this Way great Quantities of Blood. in one Motion

and sometimes two Motions every Day for Seven Days together, in Opposition to the most efficacious Restingents. As it made its regular Return by vast Profusions of Blood from the *Intestines*, in the Beginning of *October* following, to the End of the first Period of Seven Days, without Gripings, or any such uneasy Sensations. Thus, again, it kept as orderly Returns about the Vernal and Autumnal Equinoxes of the Years 1732, 1733, with vast Profusion of Blood by Stool, for the usual Term of Seven Days, agreeing in all Circumstances with the preceding Years. Likewise at, or very near these two grand Seasons, in the Years 1734, 1735, this habitual *Hæmorrhage* broke away by the *Kidneys and urinary Passage*; and still constantly, for these Two Years, kept its old stated Time of Seven Days, without any other Variation.

This young Man was seized in *Dec.* 1735, with the *Small-pox*, of the Distinct Kind, which produced such a Change in his Constitution, that he escaped those periodical *Hæmorrhages*, or any other spontaneous Evacuations equivalent thereto, for the Two Seasons of the Year 1736; and remained in very good Health till *Christmas* following, being above Thirteen Months free from any Symptoms of his old Eruption. But, upon *December* the 27th, without any previous Notice of Heaviness and Sleepiness, the *Hæmorrhage* returned by the *urinary Passages*; but much more favourably, and continued only Three Days. Again, on *May* the 13th following, 1737, he then felt the previous Warnings, and bled again by Urine to the 20th of the same Month; with this Difference, that for Three Days the Urine was only Coffee-coloured, but afterwards, for four Days longer, every Discharge re-

resembled an Effusion of Blood from a Vein just opened. He presently recovered his Strength, even although the Air was exceeding warm at this Time; and I saw him Five Months after, very robust and healthy, and, as he told me himself, was free from all kinds of Tendency towards his old Complaint. But he had always the Appearance of too much Fullness, though I am of Opinion, that his Constitution did not suffer so much as might reasonably be imagined, from such prodigious *Hæmorrhages*. Of my own Knowledge, he had no Return of his Bleeding, or any thing like it, the ensuing Autumn; but remained perfectly well all the following Winter Season. Afterwards I had no Opportunity of making further personal Inquiries, but was informed by an intelligent Man, that in *March* 1738 this unfortunate Person got a slight Wound again, somewhere upon one of his Legs, which proved equally as difficult, with respect to the Flux of Blood, as the first Puncture in his Foot. And, whether from too strict a Restraint of the *Hæmorrhage*, or for want of Venesection, he fell into very violent Convulsions for Four or Five Days, and died in a manner like Suffocation, from too much Redundancy of Blood.

As this *Hæmorrhage* never once depended upon any other Distemper, or observed any regular Concurrence with the Revolutions of the Moon, it appears to be a very extraordinary simple *Plethora*. During the Four Years that this Flux of Blood came from the Nose and Intestines, the Urine was never of a higher Colour than Amber; nor was there any Symptom of a Fever by the Pulse, or otherways, for the whole Term of the Disorder.

January the 6th, 1743.

ON March the 26th, 1739, the Wife of Mr. *Matth. Wilkinson*, of *Long-Sutton*, in *Lincolnshire*, was tapped for an *Ascites*, proceeding from frequent *Hæmorrhages*, and a too liberal Use of small Liquors. She was between 30 and 40 Years old, of a very low Stature, and always of a weak Constitution. The Water was all taken away at one Time, and measured Five Gallons. She was very faint immediately after the Operation, and remained so for near Three Weeks after. But, by great Abstinence from Liquids, excepting *Lower's* bitter Infusion, and sometimes a Spoonful or Two of Cordial Julap, she perfectly recovered her Health again; and to a much better Degree of it, than she had enjoyed for many Years before; without any Appearance at all of a Return of the abdominal Tumour to this Day. The Water was clear, and readily turned to a strong Jelly upon heating it; and I am very certain, there was unavoidably left in the Abdomen a Quantity sufficient to prove the Existence of absorbent Vessels. Perhaps those Patients, in this Distemper, whose Water turns to a Jelly, have a better Chance to be cured by *Paracentesis*, than others, whose Discharge is more like Urine, and will never curdle by Heat. But Time, and repeated Observation, must confirm this Opinion.

Postscript against Empiricism.

NOTwithstanding the great Usefulness and Tendency of successful Observations in Physic and Surgery, to encourage Practitioners in a Perseverance in their Duty, even where the regular Prognostics stand against them; yet I cannot forbear taking this Opportunity to assert the Impossibility of any Person's obtaining a competent Knowledge of the Art of Healing, by Practice only; without a previous Knowledge of Anatomy, and the Animal Oeconomy.

Hence have I met with an old Practitioner, of very extensive Business, who had never been educated in this fundamental Qualification, and who, for want of it, was treating his Patient with Cataplasms and Fomentations, to cure that Numbness in the Thigh which is a certain Diagnostic of a *Nephritic Case*. And I have more than once seen a *Dysuria* under the Treatment of a gravelly Case, when it has plainly arisen from the *Venæ Hæmorrhoidales* having been too much swelled towards the urinary Passages. Thus, where Men are not qualified to distinguish at all upon the Symptoms of a Distemper, from some true Knowledge of the Parts concerned, they must necessarily be often mistaking the Symptoms for Distempers, and so *vice versa*, after this Manner; inso-much that their longest Practice will be little better than a Multiplication of Blunders, without much Chance to be wiser by Time and Experience.—*Huic Ratiocinationi adstipulatur Experientia, qua sine vana omnis Theoria, bella sit utcumque.*

XVI. *A Letter from Mr. Turbevil Needham,
 to the PRESIDENT; concerning certain
 chalky tubulous Concretions, called Malm :
 With some Microscopical Observations on the
 Farina of the Red Lily, and of Worms dis-
 covered in Smutty Corn.*

S I R,

Recd. Dec. 22.
 1743.

THOUGH you desired me, when I had
 the Satisfaction of waiting upon
 you at *London* some Weeks ago, to commit to Writ-
 ting the Observations I had made upon that chalky,
 alkalizate Substance, which they here apply to Manure,
 and call *Malm*; yet I purposely deferred complying
 with my Engagement, till a Review of some Parti-
 culars, which I had before observed but slightly, as
 well as some additional Remarks, which I have since
 made, should enable me to give in a more satisfactory
 Account, than I could engage to do at that time—
 This Bed of *Malm* lies in a Valley, at the Foot of a
 long Ridge of chalky Downs; extends from *Win-
 chester*, where it begins, as I have been informed,
 almost due South, about Four measured Miles; the
 Breadth not above a Quarter of a Mile; and Depth,
 at a mean Computation, about Five Foot. It is used
 in Manure for the same Purposes as Chalk is, but an-
 swers the Intent much better. It rises up in one con-
 tinued Bed, almost to the Surface; where a thin Layer
 of common Earth but just hides it in all Places, where
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continual Cultivation has not superinduced a new Soil. Horsetul, and a Species of Wild Trefoil, grows out of it very plentifully, especially the first, which sink their fibrous Roots to a considerable Depth in it: The whole Bed consists of separate detached Pieces, in the Nature of those which you have by you, and of several Dimensions, as those are, mostly long and tubular; some few round, with a small Cavity in the Centre, others quite flat, and some, as it were, excavated on one Side, as if the chalky *Lamæ* had extended themselves round a Piece of Bark; but all of them hollowed within, agreeable to their exterior Shape, except very few. I believe it may be asserted, with some Confidence, that this Valley formerly was over-run with Wood, if not wholly, at least for some considerable Length and Breadth: Wild Boars Tusks, which are known by their Length; Stags horns, and a Flint-knife, which have been found buried to some Depth, in the Malm, seem to evince as much. That Trees of considerable Dimensions have grown in it, is very evident; for, in a Drain, which they have lately made to convey the Water from the main River to the adjacent Meadows, Trees of a vast Size may be seen, at Two or Three Feet Depth, in no small Number, retaining both Shape and Substance in some measure, though much decayed, and not so compact and solid in those Parts, which have been exposed to the Water; these lie out of the Verge of this Bed of Malm, and are not consequently affected by it. Now I am much inclined to think, that these Trees, together with the rest of the Wood, might, by Age, and some Accident combining with it, have fallen; the uppermost might have served to bury
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the rest, and preserve them from a more immediate Decay, by cutting off their Communication with the exterior Air. Rains, in Process of Time, must have washed off from the adjacent Hills to some certain Distance, and deposited in the neighbouring Valley, but mixt with other heterogeneous Substances, as decayed Wood, Earth, &c. a Quantity of chalky Particles, sufficient to involve, by a continual Addition of new *Laminæ*, Roots, Trunks, Branches, Twigs, and the broken Extremities of Twigs; and tending continually to form Masses resembling the supposed Particulars. I don't now imagine, tho' I once thought so, that these chalky Particles have penetrated the Wood itself, and converted it into its own Substance, in the Nature of ordinary Petrification, except here-and-there some few particular Pieces; but I rather suppose, that the Pieces of Wood have been invested continually by additional *Laminæ*; that the first *Laminæ* must have adapted itself to, and assumed the exterior Shape, whether smooth or knotty, of the inclosed Wood; that the others have proceeded accordingly; that the Extremities have gradually rounded themselves; and that in the Interim, till they were wholly closed, the included Wood has been insensibly attenuated by the passing Moisture, and, Particle by Particle, either intirely, or in Part only, wasted away. And, though it may be objected against this Supposition, that some Pieces are intirely solid, as one of those two large Pieces is which you have by you, and has the Resemblance of White-thorn; yet these are but rarely found, and may very well be supposed to have been a Species of Wood of a more solid and durable Contexture; which might consequently

quently withstand any considerable Attenuation by Water, long enough to permit the chalky Particles to penetrate, fix, and convert it into its own Substance; while other Woods, less tenacious, insensibly waste, and are carried off by the insinuating Liquid, together with the chalky Particles, which they not only could not arrest, but prevented effectually, by a Blending and Interposition of their own Parts, from adhering to each other. — The Reasons, why I apprehend the Process of the Whole to have been in the manner described above, and answerable to my Supposition, are, first, the close Vicinity, I may almost say, Contact of the chalky Hills, upon which this Bed of Malm attends throughout the whole Line, and no farther. Secondly, That this Malm is an Alkalizate Body, in a Degree something inferior to Chalk, as I found upon a Trial, some time ago, by putting equal Portions of each into equal Quantities of double-distilled Vinegar, and measuring the Height of the Fermentation in a long cylindrical Glass. Thirdly, The Reasons, which I gave above, for supposing that this Valley formerly has been over-run with Wood. Fourthly, The Disposal of the several detached Pieces of Malm, which lie in all manner of Directions. Fifthly, The Resemblance which they bear to Roots, Trunks, Branches, Twigs, &c. Sixthly, Some additional Observations, which I have made since my Return from *London*; and those, I think, are almost decisive. In the Hollow of some of the oblong tubular Pieces, which were closed at both Ends, upon breaking them open, I found the Remains of the included Wood attenuated to a mere Thread, which, though extremely tender, I could plainly discover to be Wood, both by

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its exterior Appearance, as well as by rubbing in my Hand, in order to try if it would colour it, as decayed Wood, that has imbibed Moisture, will do. Within the *Laminae* of several, I found a fair Impression of Leaves, in no small Number, and with little Trouble: The Leaves I knew not, as not being very familiar in the Vegetable World, though they appeared to me much to resemble White-thorn-leaves in their Shape, differing in this alone, that the Impression of the fore Part of the Leaf had many small indented Cavities, equal in Size to a Pin's Point, which had been formed by small Protuberances in the Leaf itself. Some Pieces I found quite flat, as if the chalky *Laminae* had involved a Chip, and the Cavity consequently went off insensibly less towards each Extremity. Others I found, whose Cavities at the Extremities were irregularly shaped, agreeable to the jagged Ends of broken Sticks. Some, in fine, I found excavated on one Side, and convex on the other, as if the *Laminae* had surrounded a Piece of Bark. These are the chief Observations which I have hitherto made, and which, I hope, are sufficient either to fix the Point where I have placed it, or to enable you to draw better Consequences; a Communication of which, at your Leisure, would please me much more than my own Supposition does, and inhance my past Obligations. I cannot say, that I am so thoroughly satisfied with what I have advanced, as to judge it unquestionable; though I am sensible, that the finding of several Masses of Malm, the Structure of which is not reducible to, nor explicable by, this Scheme, is no Objection to it; because, as every one knows the Tendency which chalky Particles have to dispose them-

themselves in *Laminae*; so these *Laminae* may involve Bodies of different Kinds, as Parts of the fibrous Roots of Weeds, small Seeds, or the like; may assume their Shapes, increase continually in Bulk, and insensibly raise the Height of the Bed, where they are first formed. Perhaps an Examination of those Pieces of *Malm*, which you have by you, may enable you to form a better Judgment of the Whole.

I beg Leave to add a few Particulars relating to some microscopical Discoveries I have lately made. Upon viewing an Infusion of the *Farina Fœcundans* of the *Lilium rubrum flore reflexo* in common Water, I thought I perceived some Alteration in several of these minute Bodies, as if the outward Shell or Husk had, at a small lateral Orifice, shed a long Train of Globules adhering to each other, and enveloped in a filmy Substance. I, immediately upon this, applied some fresh *Farina*, adapted my Microscope before-hand, with the Tip of my Brush dropped a small Globule of Water upon the Object, and in a few Seconds, I plainly perceived a Rope of exceeding small Globules to be ejaculated with some Force from within, and contorting itself from one Side to the other, throughout the whole Line, during the time of Action, which does not last above a Second or Two, and is to be expected from a few only of these farinaceous Globules. These emitted Particles are very different from the small Globules of Oil, with which the *Farina* of the Lilly abounds; for these diffuse themselves equally on all Sides, while those, on the contrary, go off in one continued Train, like the ejected Pulp of a roasting Apple; and are involved in a filmy Substance, as the Eggs of some

aquatic Insects are. I have since chosen the *Farina* of a Pompon to repeat this Experiment upon, which is not of an oily Nature; and, upon account of its Size, may be conveniently observed with the Second Magnifier, where I have the Advantage of a larger Field. I viewed some few of these also out of the many farinaceous Globules, which were within the Area of my Microscope, with the same Success, and yet greater Pleasure: For I could plainly perceive, during the time of Action, by Two or Three lucid Specks in the Centre of the Globule, which continually shifted their Places, an intestine Commotion within the farinaceous Corpufcle, and a stronger Ejaculation of the emitted Particles. Mr. *Chambers* says in his Dictionary, that no Alteration has been observed upon the Infusion of the *Farina* in Water: But this, I apprehend, is owing to the Observer's not being ready with his Microscope, and present at the time of Action, which is almost instantaneous; and, as the Orifice at which these Particles emerge, is but small, it produces no very sensible Alteration in the Globule itself.

Upon opening lately the small black Grains of smutty Wheat, which they here distinguish from blighted Corn, the latter affording nothing but a black Dust, into which the whole Substance of the Ear is converted; I perceived a soft white fibrous Substance, a small Portion of which I placed upon my Object-plate: It seemed to consist wholly of longitudinal Fibres bundled together; and you will be surpris'd, perhaps, that I should say, without any the least Sign of Life or Motion. I dropped a Globule of Water upon it, in order to try if
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the Parts, when separated, might be viewed more conveniently; when, to my great Surprize, these imaginary Fibres, as it were, instantly separated from each other, took Life, moved irregularly, not with a progressive, but twisting Motion; and continued so to do for the Space of Nine or Ten Hours, when I threw them away. I am satisfied they are a Species of aquatic Animals, and may be denominated Worms, Eels, or Serpents, which they much resemble. This, if considered, will appear to be something very singular: But I have since repeated the Experiment several times, with the same Success, and gratified others with a Sight of it. I hope these few Discoveries will prove as agreeable to you, as they were to him, who begs Leave to subscribe himself,

S I R,

*Your most obedient
humble Servant,*

Twiford, Aug. 11. 1743.

Turbervill Needham.

E R R A T A.

In N^o 468. p. 374. l. 22. for *nor ever* read *but*: And ib. l. 23. for *even* read *ever*.

N^o 470. p. 551. under the Crown, for E. L. read E L.

N. B. At the End of N^o 465. after p. 183. Two Leaves were cancelled; and therefore N^o 466. begins at Page 192. and not with 189.

N. B. The spare *Titles* to N^o 446. and to N^o 467. may be cancelled.

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I N D E X

T O T H E

Forty-second VOLUME

O F T H E

Philosophical Transactions.

For the YEARS 1742, and 1743.

N. B. In *n.* 467, the Pages are to be alter'd with a Pen; *p.* i. answers to *p.* 281, and so on to *p.* xviii. which must be made *p.* 298.

A.

A *Bada*, the Rhinoceros of *Bontius*, *n.* 470, *p.* 526.
Alo, Mercorolog. Obsl. there, *n.* 466, *p.* 243.

Æschynomene, a sort of sensible Plant, *n.* 462. *p.* 6.

Arr, Method of changing it in the Holds of Ships, *n.* 462.
p. 42.

— foul and stinking, a Method to extract it out of
 Ships, *n.* 463, *p.* 62.

— its Resistance to swift and slow Motions, *n.* 469.
p. 437.

Allmand, (M. —) of the *Polypus* Insect, *n.* 466, *p.* 219.

Altars; the most antient, *n.* 471, *p.* 584.

Ambe of *Hippocrates*, improved by *M. le Cat*, *n.* 469, *p.* 387,
 391.

— *Mr. Freke's* Improvement of it, *n.* 470, *p.* 556.

Anatomico-practicæ Obsl. 2. *D. Basteri*, *n.* 466, *p.* 277.

Aneurisma falsum non pulsans, cruorem continens, *n.* 466, *p.*
 275.

Animal-

I N D E X.

- Animalcules* found in Mushrooms, *n.* 471, *p.* 597.
Anonymous Letter, from Cambridge, concerning the *Polypus*, *n.* 466, *p.* 227.
Antidote to the *Indian Poison*, in the *West Indies*, *n.* 462, *p.* 2.
Aquatic Animal, a strange, *n.* 469, *p.* 419.
Arateorum Syntagma citat. *n.* 466, *p.* 222.
Aristotle, Obs. that *Wasp*s and *Scorpions* live when cut asunder, *n.* 466, *p.* 230.
Ascites cured by Tapping, *n.* 471, *p.* 628.
Asphaltis, Experiments upon the Water of, *n.* 462, *p.* 48.
Astronomica Obs. *Pekini*, 1740, and 1741. *n.* 468, *p.* 306.
Austin (St.) Obs. that the *Scolopendra* lives after being cut into many Pieces, *n.* 466, *p.* 231.

B.

Baker (Henry) of a strange *Aquatic Animal*, *n.* 469, *p.* 416.

of the Girl who speaks without a Tongue, *n.* 464, *p.* 143.
of a *Polype* dried, and the Manner of drying it, *n.* 471, *p.* 616.
Basilica, stone Pillars, *n.* 471, *p.* 583.
Balance-swing for crooked Children, *n.* 462, *p.* 20.
Ball of Fire seen, Dec. 11. 1741. *n.* 462, *p.* 1. *ib.* *p.* 15.
n. 463, *p.* 58. *ib.* 60. *n.* 464, *p.* 138.
Banjer, M. D. (Henry) of an extraordinary *Hæmorrhage*, and of an *Ascites* cured by Tapping, *n.* 471, *p.* 628.
Barbadoes, a *Zocphyton* there, like a Marigold-Flower, *n.* 471, *p.* 591.
Barometrorum altitudinum differentie, *n.* 464, *p.* 115.
Bases of the Cells, wherein *Bees* deposite their Honey, *n.* 471, *p.* 565.
Baster M. D. (Job) Obs. 2. *Anatomico-practica*, *n.* 466, *p.* 277.
Eces; of the *Bases* of the Cells, wherein they deposite their Honey, *n.* 471, *p.* 565.
Beighton (Henry) Meteorological Obs. at *Coventry*, *n.* 466, *p.* 243.
Bell (Geo.) Account of *Will. Payne's* Case, *n.* 462, *p.* 54.
Bellows, *Hessian*, improved, *n.* 463, *p.* 65.

I N D E X.

- Beetle* (Honourable *Charles*) of the *Polypus* Insect, *n.* 219. *n.* 467, *p.* 282.
Bethel, a Stone Pillar, *n.* 471, *p.* 583.
Bewick (*Sylvanus*) of the Bones of a Woman growing and flexible, *n.* 470, *p.* 488.
Bovis M. D. (*Johann*) de *Fructibus Aëris in f. b. m.* 31. 1736, and *Obf.* 25. 1741, *n.* 472, *p.* 612.
Breuer (*Joh. Ambrosius*) de *Succino*, *n.* 468, *p.* 372.
Births and Burials at Briggworth; a Table of, *n.* 474, *p.* 13.
Bodington (*Benjamin*) of the Girl, who speaks without a Tongue, *n.* 464, *p.* 143.
Bones of a Woman growing soft and flexible, *n.* 470, *p.* 488.
 — or Stones in the Head of Fish, *n.* 462, *p.* 29.
Bonnet, M. D. (*Charles*) of a Water-worm, which, being cut in Pieces, becomes so many perfect Worms, *n.* 467, *p.* 295.
 ————— new *Obf.* upon Insects, *n.* 470, *p.* 458.
Bridgnorth, a Topographical Account of, *n.* 464, *p.* 127.
Broman (Mr. —) *Obf.* Meteorolog. at *Hudickfsal*, *n.* 460, *p.* 243.
Buffon (M. —) of the *Polypus* Insect. *n.* 466, *p.* 219.
Burgefs M. D. (*Isaiah*) Antidote to the *West Indian Poison*, *n.* 462, *p.* 2.
Burton, M. D. (*Will.*) two Histories of internal Cancers, *n.* 464, *p.* 99.

C.

- Calculus prægrandis à Muliere cum Urina excretus*, *n.* 468, *p.* 363.
Calf, Sea; or *Phoca*, *n.* 469, *p.* 383.
Cambridge, a Letter from thence, concerning the *Ichypus*, *n.* 466, *p.* 227.
Campbell, of *Kernan*, (Rev. *Rob.*) of a Man who lived 18 Years on Water, *n.* 466, *p.* 240.
Cancers, internal; two Histories of; *n.* 464, *p.* 99.
Canterbury, Diary of the Weather there, 1734, *n.* 466, *p.* 244.

Carolina,

I N D E X.

Carolina, South; Statical Experiments, and Meteorological
Obs. there, *n.* 470, *p.* 491.

Cartilages, of the Structure, and Diseases of Articulating,
n. 470, *p.* 514.

Castilhoneus (*Joh.*) *de Polynomio*, *n.* 464, *p.* 91.

Cat. M. D. (*Claud. Nic. le*) of the *Ambe* of *Hippocrates*, *n.*
469, *p.* 387.

—— Account of his *Traité des Sens*, *n.* 466, *p.*
264.

a Machine for dressing Patients, who have
any Ailment on the *Back*, or *Os Sacrum*, *n.* 468, *p.*
364.

Caterpillars, new Obs. on them, by *M. Bonnet*, *n.* 470, *p.*
458.

Catharine's Island (*Saint*) near *Brasil*, Obs. Eclipse of the
Moon, *Dec.* 21. 1740, *n.* 462, *p.* 18.

Catlyn, (*John*) Calculation of the Transit of *Mercury* over
the Sun, *Oct.* 25. 1743, *n.* 466, *p.* 235.

Cells; of the *Bases* of them, wherein *Bees* deposit their
Honey, *n.* 471, *p.* 565.

Celsius (*Andreas*) Meteorolog. Obs. at *Upsal*, *n.* 466, *p.* 243.

Champignons, of their Seeds, *n.* 471, *p.* 595.

Charge, how much Powder is kindled in the Explosion of
one, *n.* 465, *p.* 172.

Child of a monstrous large Size at *Rouan*, *n.* 471, *p.* 627.

—— a *Lusus Naturæ* in one, *n.* 464, *p.* 152.

Churchill River in *Hudson's Bay*, various Obs. *n.* 465. *p.*
157.

Clock-makers Company, their Standard *Yard*, *n.* 470, *p.* 550.

Cold, the Effects of it in *Hudson's Bay*, *n.* 465, *p.* 157.

Collins (*Bartholomew*) his Case, *n.* 464, *p.* 99.

Celure, its Position in the antient Sphere, *n.* 466, *p.* 221.

Cometæ Obs. *Viennæ*, *Feb.* 1743, *n.* 470, *p.* 457.

Committee of the Royal Society, to examine some Que-
stions in Gunnery, *n.* 465, *p.* 172.

Cooke (*Benj.*) of the Fire-ball seen *Dec.* 11. 1741, *n.* 462.
p. 25.

Corke (*Robert* Lord Bishop of) of an antient Temple in Ire-
land,

I N D E X.

- land, like *Stone-henge*, and of a *Stone Hatchet* of the *an-*
tient Irish, *n.* 471, *p.* 581.
Corn, *Smutty*; Worms Obs. in it, *n.* 471, *p.* 63.
Cornus (Rev. Mr. *Rick.*) Account of *Bridgnorth*, *n.* 464, *p.*
 127.
Corromondel near Mount *Sinai*, hot Spring there, *n.* 462, *p.* 52.
Coventry Meteorolog. Obs. there, *n.* 466, *p.* 243.
Coxæ articuli suppuratio, cum secessione capitis femoris, *n.* 466,
p. 274.
Cradle, an hanging; by M. *le Cat*, *n.* 468, *p.* 364.
Crooked Children, a Swing for them, *n.* 462, *p.* 20.
Cutting (*Margaret*) who speaks, though she had lost her
 Tongue, *n.* 464, *p.* 143. her Letter, *ib.* *p.* 150.
Cyrillus M. D. () Meteorolog. Obs. at *Naples*, *n.* 466,
p. 244.

D.

- Daniells* (*Gabriel*) of a Girl, who speaks without a Tongue,
n. 464, *p.* 149.
Darlington Meteorol. Obs. there, *n.* 466, *p.* 244.
Dead-Sea, Analysis of its Waters, *n.* 462, *p.* 48.
Dennis (*John*) of the Girl, who speaks without a Tongue,
n. 464, *p.* 149.
Desaguliers (*John Theophil.*) LL. D. Obs. on Electricity,
n. 462, *p.* 14.

 Conjectures concerning Electricity, and
 the Rise of Vapours, *n.* 464, *p.* 140.
 his Improvement of the *Hessian Bellows*,
n. 463, *p.* 65.
Dioscorides, an Obs. on Poisons, *n.* 462, *p.* 7.
Dod, M. D. (*Pierce*) of bloody Urine in the *Small-pox*,
n. 470, *p.* 559.
Doppelmayr (*Joh. Gabriel*) Diaries of the Weather,
n. 466, *p.* 245.
Douglas, M. D. (*James*) publish'd the first true Figure of
 a *Rhinoceros*, *n.* 470, *p.* 527.
Dropsy, an extraordinary Case of a, *n.* 466, *p.* 223.
Durer (*Albert*) his Figure of the *Rhinoceros* false, *n.* 470,
p. 524.

Eames,

I N D E X.

E.

Eames, (John) his Account of *Jac. Theod. Klein Hist. natur.*

Piscum, n. 462, p. 27.

Farthen Ware, a Gold-colour'd Glazing for, n. 465, p. 188.

Earthquakes felt at *Leghorn*, Jan. 16.—27. 1742, n. 463, p. 77.

———— from 1727 to 1741 in *New England*, n. 462, p. 33.

Eclipse of the Moon, Dec. 21, 1740, in *New England*, n. 471, p. 577. At *St. Catharine's* in *Brasil*, n. 462, p. 18.

———— *Oct.* 22. 1743, *Obs.* at *London*, n. 471, p. 580.

Eclipsis Lunæ, Jan. 2 1741, *Obs.* *Pekini*, n. 468, p. 309.

Edinburgh, *Diaries of the Weather* there, n. 466, p. 245.

————, the *Pox* there, 1497, n. 469, p. 420.

Eels in *Vinegar*, *Obs.* on the *Mouths* of them, n. 469, p. 416.

Egedins (*Hans*) the natural *History of Greenland*, n. 471, p. 607.

Electricity, *Obs.* by *Dr. Desaguliers*, n. 462, p. 14.

———— *Conjectures* concerning it, n. 464, p. 140.

Electrics per se, n. 462, p. 14.

Ethiopic Stones used as *Knives*, n. 471, p. 587.

Exchequer, the *Standard Weights and Measures* examin'd, n. 470, p. 544, 551.

Explosion heard in the *Air*, Dec. 11, 1741, n. 462, p. 1.

F.

Farina of the Red Lily, *Microscop.* *Obs.* on it, n. 471, p. 63.

Du Faye (*Mr.*) two *Sorts of Electricity*, n. 464, p. 140.

Feculig, concerning it, n. 466, p. 264.

Femoris Capitis Scessio per Suppurationem *Coracæ*, n. 466, p. 274.

Ferguson (*John*) lived 18 *Years on Water*, n. 466, p. 240.

Fire-ball seen Dec. 11, 1741, n. 462, p. 1. *ib.* p. 25. n. 463, p. 58. *ib.* 60. n. 464, p. 158.

Fish, a *Method of preparing Specimens*, by drying their *Skins*, n. 463, p. 57.

———— an *Essay towards their natural History*, by *M. Klein*, n. 462, p. 27.

———— *Bones*, or *Stones* in their *Heads*, *ib.* p. 29.

I N D E X.

Fish, their Hearing, *n.* 462, *p.* 28.

Hoccorum nivalium Figura, *n.* 464, *p.* 114.

Fluxions, an Account of Mr. *Mac Laurin's* Book, *n.* 468, *p.* 325. *n.* 469, *p.* 403.

Folkes, Esq; (*Martin*) Account of the Proportions of the *English* and *French* Measures and Weights, *n.* 465, *p.* 185.

———— of the *Polypus* Insect, *n.* 466, *p.* 219.

———— Account of the *Fresh-water* POLYPUS, *n.* 469, *p.* 422.

Report of the Comparison of the Standard of Weights and Measures at the *Royal Society* with those at the *Exchequer*, *Guildhall*, *Founders-Hall*, and the *Tower*, *n.* 470, *p.* 541.

Formica-Lee, Obl. on it, by *M. Bonnet*, *n.* 470, *p.* 463.

Forth, Esq; (*Henry*) Obl. of the Weather, *n.* 466, *p.* 244. *Founders-Hall*, the Standard Weights examin'd, *n.* 470, *p.* 541.

Fritz (Rev. P.) *Olſ. Cometæ Viennæ*, Feb. 1743, *n.* 470, *p.* 457.

Freke, (*John*) an Instrument for reducing a dislocated Shoulder, *n.* 470, *p.* 556.

Fremont Colnad (*Cla.*) of Earthquakes at *Leghorn*, Jan. 1742, *n.* 463, *p.* 77.

Fresh-water Polypus, ſeveral Papers of it, *n.* 467, *p.* 281. *n.* 469, *p.* 422.

G.

Gardiner (Mr.) his Caſe, Stones in Bags in the Bladder, *n.* 462, *p.* 11.

Geoffroy (*Claud. Joſ.*) his Method of making Soap-Lees and Hard Soap, *n.* 463, *p.* 71.

———— of a Child of a monſtrous large Size, *n.* 471, *p.* 627.

Gemino Cornu extulit Urſum, *Martial. Epigr. Lib. IV. Ep. 82.* explained, *n.* 470, *p.* 537.

Geyn (*Jacob, de*) *Aratea*, citat. *n.* 466, *p.* 222.

Girard de Villars, (*M. —*) of cutting Star-fiſh, and their Radii growing again, *n.* 467, *p.* 296.

Gazing, a Gold-colour'd for Earthen Ware, *n.* 465, *p.* 188. *Comda,*

I N D E X.

- Gomda*, a Rhinoceros in the *Indian*, *n.* 470, *p.* 525.
- Gordon* (Capt. *Will.*) an Account of the Fire-ball seen
Dec. 11. 1741, *n.* 463, *p.* 58.
- Gosling* (Rev. Mr. *Will.*) of the Fire-ball seen *Dec.* 11.
 1741, *n.* 463, *p.* 60. and of Mock-suns *Dec.* 19. 1741, *ib.*
- Graham* (Geo.) Obsl. of the Transit of *Mercury* over the
Sun, *Oct.* 25. 1743, *n.* 471, *p.* 578. Eclipse of the
Moon, *Oct.* 22. 1743, *ib.* *p.* 580.
- Grandgor*, i. e. the Pox in *Edinburgh* 1497, *n.* 469, *p.* 421.
- Green*, M. D. (*John*) Account of *Hans Egedius's* Nat.
 Hist. of *Greenland*, *n.* 471, *p.* 607.
- Greenland*; its Natural History by *Hans Egedius*, *ib.*
- Gronovius*, M. D. (*John Frid.*) of preparing Specimens of
 Fish, by drying their Skins, *n.* 463, *p.* 57.
- of a Water Insect [the *Polypus*], which,
 being cut into several Pieces, becomes so many perfect
 Animals, *n.* 466, *p.* 218.
- Grubs*, Vine; Obsl. on them by M. *Bonnet*, *n.* 470, *p.* 465.
- Guettard* (M. —) of cutting Star-fish, and the *Radii*
 growing, *n.* 467, *p.* 295.
- Guildhall*, the Standard *Weights* and *Measures* examin'd,
n. 470, *p.* 548.
- Gunnery*, new Principles of, by B. R. *n.* 469, *p.* 437.
- Questions in; proposed by Dr. *Furin*, *n.* 465,
p. 172.
- Gun-powder*; its Force, *n.* 469, *p.* 437.
- H.
- Hadley* (Geo.) Abstract of Meteorolog. Obsl. from 1731 to
 1735, *n.* 466, *p.* 243.
- Hæmorrhage*; of a remarkable one frequently returning,
n. 471, *p.* 628.
- Hales*, D. D. (*Stephen*) his Ventilators or Ship's Lungs,
n. 463, *p.* 65.
- Haller*, M. D. (*Albertus*) an Account of his *Enumeratio*
Methodica Stirpium Helvetiæ indigenarum, *n.* 468, *p.* 369.
- Hamman Pharaon* Water, Analysis of it, *n.* 462, *p.* 52.
- Hammock*, M. le Cat's, for dressing Patients, who have
 Ailments on the Back or Os *Sacrum*, *n.* 468, *p.* 364.
- Hammond*,

I N D E X.

- Hammond, (Will.)* of the Girl, who speaks without a Tongue, *n.* 464, *p.* 146.
- Harl-Scop*, how to make it for medicinal Uses, *n.* 463, *p.* 71.
- Hatchet*, Stone one; of the ancient *Irish*, *n.* 471, *p.* 589.
- *Amazonian*; *Sagaris*, *ib.*
- Hearing*, concerning it, *n.* 466, *p.* 266.
- of Fishes, *n.* 462, *p.* 28.
- Hensius (Godofridus)* a Gold-colour'd Glazing for Earthen Ware, *n.* 465, *p.* 188.
- *de Disparatione Annuli Saturni*, *Ann.* 1743, 1744, *n.* 471, *p.* 602.
- Hellebore, White*; its bad Effects, *n.* 468, *p.* 378.
- Helvetiæ Stirpium indigenarum Enumeratio*, *n.* 468, *p.* 369.
- Herba Sensitiva*, its Root an Antidote to the *West Indian* Poison, *n.* 461, *p.* 4. Itself poisonous, *ib.* *p.* 6.
- Hippocrates*, his *Ambe* improved, *n.* 469, *p.* 387, 388.
- Hobson (Jos.)* of the wonderful Increase of the Seeds of Plants, *n.* 468, *p.* 320.
- Holkam in Norfolk*, a Meteor seen there, *Aug.* 1741, *n.* 465, *p.* 183.
- Holland and Westfriseland*, the Quantity of People, *n.* 468, *p.* 315.
- Hollmannus (Sannuel Christianus)* *de Differentiis Altitudinum Barometrorum*, *n.* 464, *p.* 115.
- Hondius (Hendrick)* his Figure of the *Rhinoceros* false, *n.* 470, *p.* 526.
- Horns* of the *Rhinoceros* described and figured, *n.* 470, *p.* 537, 540.
- some are double, *ib.* *p.* 537.
- Huddakfoal*, Meteorolog. Obs. there, *n.* 466, *p.* 243.
- Hudson's Bay*, Obs. of Cold, Variation, Longitude and Latitude there, *n.* 465, *p.* 157.
- Hughes (Rev. Griffith)* of a *Zoophyton* like a Marigold, *n.* 471, *p.* 590.
- Huile de Chaux*, *n.* 463, *p.* 76.
- Hunter, (Will.)* of the Structure and Diseases of articulating Cartilages, *n.* 472, *p.* 205.
- Huxham,*

I N D E X.

Huxham, M. D. (*John*) of *Polypi* taken out of the Hearts of several Sailors, just come from the *West-Indies*, n. 464, p. 123.

Hydrocephalo, *Obs. de singulari*, n. 466, p. 278.

I.

Jesu; *Patrum, e Soc. Obs. Astronomicæ, Pekini*, 1741, n. 468, p. 306.

Jewels, the polishing them practised before *Moses's* Time, n. 471, p. 588.

Indian Poison, an Antidote to it, n. 462, p. 2.

Infante, *Obs. de*; *qui Saccum Aqua plenum, ab Osse Sacro usque ad Talos propendentem, habuit*, n. 466, p. 277.

Insect, a Water, that being cut into Pieces, becomes so many Animals. See *Polypus*, *ib.* p. 218.

— new *Obs.* on them by M. *Bonnet*, n. 470, p. 458.

— which are multiplied by Cuttings or Slips, *ib.* p. 468.

Instrument, Sir *Is. Newton's*, for *Obs.* the Moon and fixt Stars at Sea, n. 465, p. 155.

Jovis Satellitum Immersiones & Emerfiones Obs. Pekini, 1741, n. 468, p. 306.

Iron Instruments in Use in *Egypt* before *Moses's* Time, n. 471, p. 588.

— a Pen of; *Jeremiah* xvii. 1. *ib.* p. 589.

Jurin M. D. (*James*) his Questions in Gunnery, n. 465, p. 172.

Jussieu, M. D. (*Bernard de*) of cutting Star-fish, and their *Radii* growing again, n. 467, p. 295.

K.

Kent, a Diary of the Weather there, n. 466, p. 243.

Kerseboom (*Will.*) Extract of his second and third Treatise of the Quantity of People in *Holland*, &c. of the Lives of *Widows*, and Duration of Marriages, n. 468, p. 315.

Klein (*Jac. Theod.*) *Hist. natur. Piscium promovendæ Missus* I. n. 462, p. 27.

Knives, sharp, (*Josh.* v. 2, 3.) i. e. *Knives* of sharp Stones, n. 471, p. 587.

L.

Lapilli in Craniis Piscium, n. 462, p. 29. *Lapis*

I N D E X.

- Lapis infernalis*, a Sort of; *n.* 463, *p.* 72.
Latham, M. D. (Rev. *Ebenezer*) of the Position of the
Colure in the *antient Sphere*, *n.* 466, *p.* 221.
Latitude, Obs. in *Hudson's Bay*, *n.* 463, *p.* 157, 169.
Laurin. See *Mac Laurin*.
Leeuwenhoeck (M. —) of the *Polypus Insect*, *n.* 466, *p.* 220:
Legge (Honourable *Edward*) Obs. of an Eclipse of the
Moon, Dec. 21. 1740, *n.* 462, *p.* 18.
Leghorn, Earthquakes felt there, *Jan.* 16.—27. 1742,
n. 463, *p.* 77.
Le Prectus M. D. (*Antonius*) de *Calculo prægrandi à Muliere*
cum Urina excreto, *n.* 468, *p.* 363.
Lightning, extraordinary Effects of, *n.* 464, *p.* 136.
Lienis Abscessus per Vulvam facta Puris Excretionis anatus,
n. 466, *p.* 273.
Lilly, Red; Microscop. Obs. on its *Farina*, *n.* 471, *p.* 63.
Lime, Oil of, *n.* 463, *p.* 76.
Lining, M. D. (*John*) Statical Experiments, and Meteorological
 Obs. in *S. Carolina*, *n.* 470, *p.* 491.
Longitude, Obs. in *Hudson's Bay*, *n.* 463, *p.* 157, 169.
Lord (Rev. *Mr. Tho.*) of Worms, whole Parts live, when
 cut asunder, *n.* 470, *p.* 522.
Lovell (*Tho. Lord*) of a Meteor seen near *Holkam*, *Aug.*
 1741, *n.* 465, *p.* 183.
Luna obtexit stellam n in 8, *Feb.* 22. 1741, *Pekini*, *n.* 468,
p. 312.
 ————— μ in II, *Feb.* 24. 1741, *ib.* *p.* 313.
 ————— *Satellitem 3ⁱ*. *Apr.* 20. 1741, *ib.*
 ————— attigit ψ , *Apr.* 20, 1741, *ib.*
 ————— obtexit *Stellam in Cauda Ceti*, *ib.* *p.* 314.
Lunæ Eclipsis, *Jan.* 2. 1741, *Pekini*, Obs. *n.* 468, *p.* 309.
Lunden Meteorolog. Obs. there, *n.* 466, *p.* 243.
Luxations of the Arm with the Shoulder, how reduced,
n. 469, *p.* 396.
 M.
Mackay (Mr. —) of the *Pox* in *Edinburgh*, 1497, *n.* 469,
p. 420.
Mac Laurin (*Colin*) of the *Bases* of the Cells, wherein *Bees*
 depolite their *Honey*, *n.* 471, *p.* 565.

I N D E X.

- Alc Laurin*, an Account of his Treatise of *Fluxions*, *n.* 468, *p.* 325. *n.* 469, *p.* 423.
- Magnetic Needle*, Obs. of its Declination in *Hudsen's Bay*, *n.* 465, *p.* 157.
- Mollere*, the *Upright*, of the wonderful Increase of its Seeds, *n.* 468, *p.* 320.
- Molin*, a Sort of *Osteocolla*, *n.* 471, *p.* 634.
- Marriages*, a Calculation of their Duration, *n.* 468, *p.* 315.
- Martial*, Epigr. Lib. IV. Ep. 82. *Gemino Cornu* of a *Rhinoceros* explained, *n.* 470, *p.* 537.
- Mafin* (*Christoph.*) of a Fire-ball seen, and great Explosion heard, Dec. 11. 1741, *n.* 462, *p.* 1.
- Maischak*, Hebrew Pillars of Stone, *n.* 471, *p.* 585.
- Mead*, M. D. (*Rich.*) an Obs. on Poisons, *n.* 462, *p.* 7.
- Measures and Weights*, the Standards at the *Exchequer*, *Guildhall*, *Founders-hall*, and the *Tower*, compared, *n.* 470, *p.* 541.
-
- the Proportions of the *English* and *French*, *n.* 465, *p.* 185.
- Medical Cases*, two remarkable, *n.* 471, *p.* 628.
- Medico-chirurgica Obs. variae*, à *Joh. Dan. Schlichting*, *n.* 466, *p.* 270.
- Medioburgi*, Obs. *Roris decidui* & *Fig. Floccorum nivalium*, *n.* 464, *p.* 114.
- Melon-seeds* 42 Years old, *n.* 464, *p.* 115.
- Mercury* Transited over the *Sun*, *Oct.* 31. 1736, *n.* 471, *p.* 622. *Apr.* 21. 1740, *ib.* 572. *Oct.* 25. 1743, *ib.* *p.* 578. *n.* 466, *p.* 235.
- Meteor* seen near *Holkham* in *Norfolk*, *Aug.* 1741, *n.* 465, *p.* 183.
-
- at *Peckham*, Dec. 11, 1741, *n.* 464, *p.* 138.
- Meteorologica Ephemerides Romanæ*, 1741, *n.* 466, *p.* 193.
- Meteorological* Obs. for 1731, 1732, 1733, 1734, and 1735, *n.* 466, *p.* 243.
-
- in *S. Carolina*, *n.* 490, *p.* 491.
- Methodus Plantarum D. Halleri*, *n.* 468, *p.* 381.
- Michei*, M. D. (—) of the Seeds of Mushrooms, *n.* 471, *p.* 599.

I N D E X.

- Microscopical* Obs. on the *Farina* of the *Red Lily*, and of
Worms in *smutty Corn*, *n.* 471, *p.* 639.
- Middleton* (Capt. *Christoph.*) the Effects of Cold; Obs. of
 the Longitude, Latitude, and Declination of the Mag-
 netic Needle in *Hudson's Bay*, *n.* 465, *p.* 157.
- Miles* (Rev. *Hen.*) of an extraordinary Warmth in the
 Air in *Jan.* 1741-2, *n.* 462, *p.* 20.
- Obs. of the Mouths of *Riels* in Vinegar,
 and of a strange aquatic Animal, *n.* 469, *p.* 416.
- *Parhelia* seen *Dec.* 19. 1741, *n.* 462, *p.* 46.
- Miller* (*Joseph*) a Catalogue of 50 *Plants* for the Year
 1740, *n.* 471, *p.* 620.
- Milner*, M. D. (*Tko.*) of a Meteor seen *Dec.* 11. 1741,
n. 464, *p.* 138.
- Milward*, M. D. (*Edward*) of an Antidote to the *Indian*
Poison in the *W. Indies*, *n.* 462, *p.* 2.
- Mimosa arboriscens Americana*, &c. *flore albo*, *n.* 462, *p.* 6.
- Mock-Suns.* See *Parhelia*.
- Moon* and six Stars, an Instrument to observe their Dis-
 tances at Sea, *n.* 465, *p.* 155.
- eclipsed *Dec.* 21. 1740, in *New England*, *n.* 471,
p. 577.
- *Oct.* 22. 1743, Obs. at *London*, *ib.* *p.* 580.
- Mors*, *Tumuli sepulchrales*, there, *n.* 464, *p.* 134.
- Moslyn*, Bart. (*Sir Tko.*) of a golden *Torques*, *n.* 462,
p. 24.
- Mushrooms*; of their Seeds, by Mr. *Pickering*, *n.* 471,
p. 593. By Mr. *Watson*, *ib.* *p.* 599.
- Animalcules of the Maggot, or Fly-kind,
 found in them, *ib.* 597.
- N.
- Naples*, Meteorolog. Obs. there, *n.* 466, *p.* 244.
- Needham* (*Turbervil*) concerning *Malin*, and some Microsco-
 pical Obs. on the *Farina* of the *Red Lily*, and of *Worms*
 discovered in *smutty Corn*, *n.* 471, *p.* 634.
- Needle*, Magnetic, its Declinations, Obs. in *Hudson's Bay*,
n. 465, *p.* 157.
- Negro Poison*, and its Antidote, *n.* 462, *p.* 6.

I N D E X.

New-England, Transit of \S over \odot , *Apr.* 21. 1740, and ϵ
eclipsed *Dec.* 21. 1740, Obs. at *Cambridge*, *n.* 471, *p.* 572.
————— *Journal of Earthquakes* from 1727 to 1741.

n. 462, *p.* 33.

Newton (Sir *Izaak*) an Instrument for obs. the Moon's Di-
stance from the fixt Stars at Sea, *n.* 455, *p.* 155.

Navis Figuræ, *n.* 464, *p.* 114.

Non-clebr is, *n.* 462, *p.* 14.

Norimberg, Diaries of the Weather there, *n.* 466, *p.* 245.

Nottcutt, (*Will.*) of the Girl, who speaks without a Tongue,
n. 464, *p.* 146.

Nourse, (*Edward*) of Stones found in Bags in the *Bladder*,
n. 462, *p.* 11.

O.

Oak-trees struck with Lightning in a Spiral, *n.* 464, *p.* 137.

Obelisks in *Egypt*, their Origin, *n.* 471, *p.* 587.

Oil of Lime, *n.* 463, *p.* 76.

Osteocolla, a Sort called *Malm*, *n.* 471, *p.* 634.

P.

Pack, M. D. (*Christoph.*) Diary of the Weather for 1734,
n. 466, *p.* 244.

Padua, Meteorolog. Obs. there, *n.* 466, *p.* 245.

Parhelia seen *Dec.* 19. 1741, *n.* 462, *p.* 46, *n.* 463, *p.* 61.

Parsons, M. D. (*James*) Account of M. le Cat's *Tr. ité des*
Sens, *n.* 466, *p.* 264.

————— of the *Phoca*, *Vitulus marinus*, or Sea-Calf,
n. 469, *p.* 383.

————— natural History of the *Rhinoceros*, *n.* 470,
p. 523.

Payne, (*Wm.*) Stones in his Kidneys and Bladder, *n.* 462,
p. 54.

Pedini, (*Pusqual R.*) Remarks on the Earthquakes felt in
Leghorn, *Jan.* 1741, *n.* 463, *p.* 77.

Pekin Obs. *Astronomiæ*, 1740 & 1741, *n.* 468, *p.* 306.

Pen of Iron, *Jerem.* xvii. 1, *n.* 471, *p.* 589.

Pendula, num *vi aliqua centrifuga perturbentur*? *n.* 468, *p.* 299.

Perry, M. D. (*Charles*) Analysis of the Water of the *Dead-*
Sea, of the hot Spring near *Tiberiades*, and the *Hamman*
Pharoan Water, *n.* 462, *p.* 48.

I N D E X.

- Petre*, (Robert James Lord) of some extraordinary Effects of Lightning, *n.* 464, *p.* 136.
- Phoca*, or Sea-Calf, *n.* 469, *p.* 383.
- Pickering*, (Rev. Roger) of the Seeds of Mullrooms, *n.* 471, *p.* 593.
- Pillars* of Stone, their ancient Use, called *Mutsebah*, *n.* 471, *p.* 583.
- Plint* (Rev. Matthias) Journal of Earthquakes in New-England, *n.* 462, *p.* 33.
- Plantarum Methodus* D. Halkri, *n.* 468, *p.* 381.
- Plants*, a Catalogue of 50 for the Year 1740, by Jof. Miller, *n.* 471, *p.* 620.
- Pleuritis suppurata per Vulvam Puellæ excreta*, *n.* 466, *p.* 273.
- Poison*, West-Indian, an Antidote to it, *n.* 462, *p.* 2.
- Polenus* (Johannes Marchio) *num* *Pendula* *vi aliqua centri-fuga perturbentur* ? *n.* 468, *p.* 299.
- Diaries of the Weather, *n.* 466, *p.* 245.
- Polishing* Jewels practised before-Moses's Time, *n.* 471, *p.* 588.
- Polynomic*, *de* ; Joh. Castillionens, *n.* 464, *p.* 91.
- Polypi* taken out of the Hearts of several Sailors, just come from the West-Indies, *n.* 464, *p.* 123.
- Polypus*, Fresh-water, an Insect, which, being cut in Pieces, becomes so many perfect Animals, *n.* 466, *p.* 218, 220. *ib.* *p.* 227. *n.* 467, *p.* 281. *n.* 469, *p.* 422. *n.* 470, *p.* 510.
- Figures of it, *n.* 469, *p.* 434.
- dried ; Obs. on it, and the Manner of drying, *n.* 471, *p.* 616.
- Pox* in Edinburgh 1497, *n.* 469, *p.* 420.
- Pucerons*, Obs. on them, by M. Bonnet, *n.* 470, *p.* 465.
- Q.
- Quadrant*, Sir Isaac Newton's, *n.* 465, *p.* 155.
- R.
- R.* (B.) an Account of his Book of Gunnery, *n.* 469, *p.* 437.
- See Robins.
- Reaumur* (M. —) Memoir of the *Polypus*, *n.* 466, *p.* 227. *n.* 467, *p.* 292.

I N D E X.

Revillas (Didacus de) excerpta ex Ephemeridibus Meteorologicis Romanis, 1741, n. 466, p. 193.

———— of the Rain at Rome 1735, n. 466, p. 245.

Rhinoceros, the natural History of him, n. 470, p. 523.

———— *Albert Durer's Figure of him false*, *ib.* p. 524.

Richmond, Lenox, and Aubigny (Charles Duke of) of the *Polypus*, n. 470, p. 510.

Rixtel (John van) Extract of Mr. *Wm. Kerseboom's* second and third Treatise concerning the Quantity of People of *Holland, &c.* and of the Lives of *Widows*, and the Duration of *Marriages*, n. 468, p. 315.

Robert Lord Bishop of Corke, of an *antient Temple* in *Ireland*, like *Stone-henge*, and of a *Stone Hatchet* of the *antient Irish*, n. 471, p. 581.

Robins (Benj.) an Account of his Book of *Gunnery*, n. 469, p. 437.

Romæ Obs. Meteorologicæ, 1741, n. 466, p. 193.

Rore deciduo de, *Obs.* n. 464, p. 112.

S.

Sagaris of Xenophon, an *Hatchet*, n. 471, p. 589.

Satellitum Jovis Obs. Astronom. Pekini, 1741, n. 468, p. 306.

Saturni, de Disparitione Annuli Ann. 1743, 1744, n. 471, p. 602.

Saxum acutum Ovid. Fast. 4. explained, n. 471, p. 588.

Schlichting (Job. Dan.) Obs. Medico-chirurgicæ variae, n. 466, p. 270.

Scolopendra lives some time, though cut into many Pieces, n. 466, p. 231.

Scott, Elq; (Geo. Lewis) his Report of the Committee of the Royal Society, appointed to examine some Questions in *Gunnery*, n. 465, p. 172.

Sea-calf, or Phoca, n. 469, p. 383.

Seeds of Plants, of their wonderful Increase, n. 468, p. 320.

Seeing, concerning it, n. 466, p. 268.

Seminis

I N D E X.

- Seminis loco Sanguis expellitur in Coitu*, *n.* 466, *p.* 273.
- Sensible-weed*, its Root an Antidote to the *West Indian Poison*, *n.* 461, *p.* 4. Itself poisonous, *ib.* *p.* 6.
- Sensitiva Herba*, its Root an Antidote to the *West Indian Poison*, *n.* 461, *p.* 4. Itself poisonous, *ib.* *p.* 6.
- Sheldrake (Tim.)* of the Steel-yard Balance-ewing, *n.* 462, *p.* 20.
- Ship*, of changing the Air in them, by Mr. Sutton, *n.* 462, *p.* 42.
- Lungs, of Dr. Hales, *n.* 463, *p.* 65.
- Skort, M. D. (Tho.)* a Case of a Dropsy, *n.* 466, *p.* 223.
- Shoulder* dislocated; an Instrument for reducing it, *n.* 470, *p.* 556.
- Luxation of it; how reduced, *n.* 469, *p.* 396.
- Slips or Cuttings*, Insects or Worms, which can be multiplied by, *n.* 470, *p.* 468.
- Plants that can be multiplied by, *n.* 470, *p.* 478.
- Shoane (Sir Hans)* the *Sensille Plant*, a Poison, its Root its Antidote, *n.* 462, *p.* 6.
- Small-pox*; bloody Urine in it, *n.* 470, *p.* 559.
- Smelling*, concerning it, *n.* 466, *p.* 266.
- Smutty-corn* Worms, Obs. in it, *n.* 471, *p.* 640.
- Soap, hard*; how to make it for medicinal Uses, *n.* 463, *p.* 71.
- *Lees*, a Method of making, for medicinal Uses, *ib.*
- Society*, the Royal, Standard *Weights and Measures* compared with those at the *Exchequer, Guildhall, Founders-hall*, and the *Tower*, &c. *n.* 470, *p.* 541, 553.
- Sole, sub*; *transitus Mercurii*, *n.* 471, *p.* 622, 572, 578.
- Southwick Meteorolog.* Obs. there, *n.* 466, *p.* 143.
- Sphere, antient*, the Position of the Colure in it, *n.* 466, *p.* 221.
- Spider*, Water; not come to Perfection, *n.* 469, *p.* 418.
- Spina Ventosa*, Obs. de, *n.* 466, *p.* 270.
- Sporing (Mr.—)* Meteorolog. Obs. at *Abbo*, *n.* 466, *p.* 243.
- Stacks.*

I N D E X.

Stackhouse (Rev. Mr.—) Account of *Bridgnorth*, *n.* 464, *p.* 127, and of some *Tumuli Sepulchrales* near it, *ib.* *p.* 134.
Standard Weights and Measures kept at the Royal Society, *n.* 465, *p.* 185.

———— compared with those kept at the *Exchequer*, at *Cuildhall*, *Founders-hall*, and at the *Tower*, *n.* 470, *p.* 541.

Star-fish their *Radii* grow again, if cut off, *n.* 467, *p.* 295.

Statical Experiments in *S. Carolina*, *n.* 490, *p.* 491.

Steel-yard Balance-swing for Children, *p.* 462, *p.* 20.

Stephens (*Joanna*) her Medicines for the Stone failed, *n.* 462, *p.* 11. *ib.* 54.

Stoeke, M. D. (*Leonardus*) de *Rore deculae*, & de *Figuris Flocorum nivalium*, *n.* 464, *p.* 112.

Stone, Ethiopic, used as Knives, *n.* 471, *p.* 587.

Ston-hatchet of the antient Irish, *n.* 471, *p.* 589.

Stone-henge; an antient Temple in *Ireland* like it, *n.* 471. *p.* 581.

Stone Pillars, their antient Use, called *Matsebah*, *n.* 471, *p.* 583.

Stones or *Bones* in the Head of Fish, *n.* 462, *p.* 29.

— found in Bags in the Bladder, *n.* 462, *p.* 11.

Succino, de; *Joh. Ambr. Beurer*, *n.* 468, *p.* 322.

Suns Mock. See *Parkeha*.

Sutton, (Mr.—) of changing the Air in Ships, *n.* 462. *p.* 42. *n.* 463, *p.* 62.

Swing the *Steel-yard Balance*, *n.* 462, *p.* 20.

T.

Tapping cured an *Afcites*, *n.* 471, *p.* 628.

Tasting, concerning it, *n.* 466, *p.* 265.

Temphs why so called, *n.* 471, *p.* 581.

— antient; in *Ireland*, like *Stone-henge*, *n.* 471, *p.* 581.

Tennison (Mrs.) *Parkeha* seen at *Canterbury*, Dec. 19. 1741, *n.* 462, *p.* 48.

Tiberiades, Analysis of the Water of the Hot Spring, near, *n.* 462, *p.* 50.

Tipula not come to Perfection, *n.* 469, *p.* 418.

Tongue, a Girl who can speak without one, *n.* 464, *p.* 150.
Torques,

I N D E X.

- Tieques*, a golden one found in *England*, *n.* 462, *p.* 24.
Tower, the *Standard Weights* and *Measures* examined, *n.* 470, *p.* 549, 555.
Trade-winds in the North, in *Hudson's Bay*, *n.* 465, *p.* 165.
Tremblay (M.—) of the *Polypus* Insect, *n.* 466, *p.* 219, *n.* 467, *p.* 283. *n.* 470, *p.* 468, 467. *n.* 470, *p.* 510.
Tree-wald (*Martin*) of the *Vegetation* of *Melon-seeds* 42 Years old, *n.* 464, *p.* 115.
Trinder (*Tlo.*) his *Cave*, *n.* 464, *p.* 104.
Tubulous chalky *Concretions*, called *Alalm*, *n.* 471, *p.* 67.
Tumuli Sepulchrales near *Brudgnorth*, *n.* 464, *p.* 114.

V.

Vapours, *Conjectures* concerning their *Rise*, *n.* 464, *p.* 140.
Variation of the *Needle*, *Obs.* in *Hudson's Bay*, *n.* 465, *p.* 157.
Veneral *Disease* in *Edinburgh*, 1497, *n.* 469, *p.* 420.
Veratrum, its bad *Effects*, *n.* 468, *p.* 378.
Viennæ, *Cometa* *Obs.* *Feb.* 1743, *n.* 470, *p.* 457.
Vinegar, *Eels* in it, *n.* 469, *p.* 416.
Vine-grubs, *Obs.* on them, by *M. Bonnet*, *n.* 470, *p.* 465.
Vision, concerning it, *n.* 466, *p.* 268.
Vitulus Marinus, or *Phoca*, *n.* 469, *p.* 383.
Upsal, *Meteorolog.* *Obs.* there, *n.* 466, *p.* 243.
Urtica Marina; a *Sort* from *Barbadoes*, *n.* 471, *p.* 591.
Urine, bloody; in the *Small-pox*, *n.* 470, *p.* 559.

W.

Wales, *Prince* of, his *Fort* in *Hudson's Bay*, various *Obs.* *n.* 465, *p.* 157.
Warmth in the *Air*, extraordinary in *Jan.* 1741-2, *n.* 462, *p.* 20.
Warwick (*Charles*) a remarkable *Conformation*, or *Lusus Naturæ*, in a *Child*, *n.* 464, *p.* 152.
Watson (*Will.*) *Remarks* concerning the *Seeds* of *Musbrooms*, *n.* 471, *p.* 599.

Obs. on *Mr. Sutton's* *Invention* for changing the *Air* in *Ships*, *n.* 463, *p.* 62.

critical *Remarks* on the *Use* of *Wind-tails*, *n.* 463, *p.* 62.

Watson

I N D E X.

- Watson (Will.)* An Account of Prof. *Haller's Enumeratio Stir-
pium Helvetiæ indigenarum*, n. 468, p. 369.
Water, a Man lived 18 Years on nothing but, n. 466, p.
 240.
 — Spider, not come to Perfection, n. 469, p. 418.
Wadler (Job. Frid.) Meteorolog. Obs. at *Witemberg*, n. 466,
 p. 243.
Weights and Measures; the Proportions of the *English* and
French, n. 465, p. 185.
 — the Standards at the *Exchequer*, *Guild-
ball*, *Founders-hall*, and the *Tower*, compared, n. 470,
 p. 541.
West-Friesland and *Holland*; the Quantity of People, n. 468,
 p. 315.
West-Indian Poison, Antidote to it, n. 462, p. 2.
Widows, a Calculation of their Lives, n. 468, p. 315.
Wind-sails, critical Remarks on the Use of them, n. 463,
 p. 62, and 65.
Witemberg, Meteorolog. Obs. there, n. 466, p. 243.
Wintthrop, Prof. Math. (*John*) of the Transit of *Mercury*,
 over the *Sun*, Apr. 21. 1740, and an Eclipse of the
Moon, Dec. 21. 1740, Obs. at *Cambridge* in *New-Eng-
land*, n. 471, p. 572.
Worm, a long one; which is to be multiplied by Cuttings
 or Slips, n. 470, p. 468.
 — a *Water*; which, being cut in Pieces, becomes so
 many *Worms*, n. 467, p. 295.
Worms, whose Parts live, after they have been cut asun-
 der, n. 470, p. 522.
 — Obs. in smutty Corn, n. 471, p. 63.
 Y.
Yard Standard; examined, n. 470, p. 541.
 Z.
Zoophyton, like a *Marigold-flower*, n. 471, p. 590.

